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STUDIES IN THE PSYCHOLOGY OF TOUCH.

By F. B. DRESSLAR, Fellow in Psychology, Clark University.

I. *Psychology of Touch in General.*

If the nervous mechanism of the body could be viewed, apart from the rest of the body, with all the nerve trunks and branches *in situ*, the form of the body would be duplicated in its externality by the network of nervous fibres and filaments which ramify and penetrate every part of the surface of the body. It is believed that each one of these fibres or filaments, however fine, connects directly with the nervous centre, and that whatever stimulus is received by each is carried to the centre over independent fibres, and that no transition of stimulus from one fibre to another is possible, however closely they may be placed together. Not only is the isolation thus perfect, but if a fibre be broken or cut, there can be no further communication established between the part of the surface where this fibre is attached and the nervous centres until the distal portion is regenerated. If the nerve trunk be submitted to mechanical pressure, the communication is severed so long as the pressure continues. Thus the whole surface of the body is in more or less complete independent connection at each point with the higher centres, and, as we shall see, this independence in structure is necessarily followed by a similar independence in function, whereby the great differences in local discriminative ability have arisen and the conditions are furnished for further dermal education.

One of the first things to notice and probably the most striking fact in the study of the skin as a sense organ, is the great difference in its extent as compared with the other organs of sense. If the sensitive surfaces of all the other sense organs were spread out in direct contact with the outer or objective world, the whole of these taken together would scarcely equal the surface of the palm of the hand. Though of course this comparison must of necessity be crude, for each organ's sensitiveness is measured by wholly different stimuli and these are incommensurable with respect to each other. Still when it is called to mind that eye, ear and nose, with all their special adaptation, are but "specialized dermal cells," the comparison is not wholly without value. Furthermore, embryology teaches that not only are the sense organs thus developed from the skin, but that the whole of the brain and the spinal cord are infolded and developed ectodermic cells. The influence of this fundamental connection between the higher centres and the skin, may be much more far-reaching than psychology has yet thought. There is at least a suggestion of the utmost importance in the philogenetic relation of the skin and the nervous centres. Some have dared to hint that the objectivation of the subjective states may here have a real causal basis. But it is safer to wait until biology, at least, has more to say on this point, before making assertions the proofs for the truth or falsity of which are still hidden in the abyss of our ignorance.

Then, too, the skin is situated and conditioned unlike any other of the sense organs, in that it is turned in all directions. The other sense organs are more or less directed toward the front and at all times are limited to a part even of their immediate environs. In a large measure this limitation is not true of the skin, though of course certain accommodations of hand and body are habitually made to facilitate and refine perception through touch. As a sense organ the skin is much less specialized than any of the other sense organs. Its range is not only larger, but it takes cognizance of the more fundamental properties of the material world. Our eyes are useless in the dark, our ears are without value when there are no vibrations in matter, but the conditions of touch remain so long as there is objective existence at all. The skin is the mother sense and out of it, all the other senses have been derived. The biologists tell us that the eye is only a developed sensory spot of the skin, and Preyer [1] even goes so far as to say that as a basis for the fact, we are constantly speaking of the qualities of things seen, in terms of qualities only felt, such as a

[1] Pflüger's Arch. 1881, p. 75.

warm color, a cold shade, etc., that the sense of sight was developed out of the sense of temperature.

If we take the amœba or any of the simpler forms of life, we can readily see that their whole life must depend on the kind of reactions they are prompted to make through skin perception. The whole surface of the bodies of such organisms presents to their environs a sort of diffused touchiness, apparently not more differentiated in one place than another. They are all eye if they see at all; all ear if they hear, all nose if they smell. Wherever they touch a particle of exterior matter, they need no further adjustment to know it as far as it is possible for them to know. There is no parallel to this in any of the other senses. As Aristotle[1] has said, we can have an animal without eyes, without the sense of smell, or taste, or hearing, but all must have the sense of touch. In fact, it is only the few in comparison that have any other sense than touch. With this primordial and far-reaching notion of touch in mind, let us examine the properties of bodies known to man through the senses. Perhaps this can be more quickly done by excluding the properties of objects derived from or through the other senses. Through the eye we get color and motion and perhaps an indistinct (until corrected and helped by the other senses) notion of distance. For the ear only sound, or vibrations within certain limits. For the nose odor, and for taste certain chemical stimulants. All the rest of our mental lumber has come through the skin either directly or indirectly. If we were asked to name the most permanent and most important properties of bodies, we should not go out of the realm of touch, including the so-called muscle sense, which is largely skin sense. We would name hardness, weight, temperature, tenacity, compressibility, shape, roughness, etc. It is easy to see that these are the fundamentals. We can easily be deceived in color, but not so with hardness. Certain interpretations of odors are very largely due to the personal factor, but weight does not exist as a variable except in degree. We have standards fast and exact for determining weight, resistance or cohesion of bodies, but not for the strength of their odor or the shades of their colors. We do not speak of a person as abnormal if his ear is not discriminative for great difference in tone, but we consider him abnormal if he could not distinguish thousands of variations in location, in quality, in temperature, etc., of objects through touch.

Then again it is perhaps helpful to see that the other senses depend largely on the skin for proof of their assertions as to

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[1] Psychology, Wallace's Trans. p. 67.

the more important of their activities. We come to judge of form by the eye, but we are in constant need of tactual measurements to correct and prevent misconception. Doubting Thomas thus becomes a sort of type for realists, all of whom insist on the most certain proofs and so rely on the tangible. As Mr. Fraser [1] says, in his suggestive essay, "In the case of the unreflective but practical thinker the question, 'What do you mean by a real world?' is openly answered and without bias by saying, 'It is a world that we can *touch*.'" "He can be persuaded that the object he *sees* before him is illusory; but if he is allowed to stretch forth his hand and can touch it and feel it there, the last remnant of doubt as to its real existence will have fled." Very rarely, if ever, do you see the fakir or sleight-of-hand performer practicing his tricks save to deceive through the special senses. Indeed, the phrase "sleight-of-hand" may have for its earlier psychological basis, the meaning now best rendered by slighting of the hand. Dr. Johnson's reply to the Berkeleyan philosopher who pressed him with his idealism, was, as everybody knows, "Throw a brick at him."

This fundamental and deep lying significance of perception through touch is seen in the commonest words having for their import to know:—*perceive* means to take thoroughly or to take hold of; *conceive* has for its fundamental and original meaning, to take hold of things together; *apprehend* is to catch hold of; *comprehend* means only to catch hold of more than one thing; *understand* means measurement of things by letting them rest on you; we *accept* a thing mentally when we take hold of it. The modern slang phrase "catch on" illustrates this same tendency to coin terms designating intellectual activity. And if the theory be true that slang is a linguistic atavism, this last phrase is full of psychological significance. Just what the full meaning of this strong tendency is, perhaps does not fully appear, yet it is certainly significant in preserving an early and for that matter an inherent theory of indisputable knowledge. "Why touch," says one writer, "the simplest and earliest sense, should in its higher forms be more than any other sense associated with the advance of intelligence, is due to the fact that tactual impressions are those into which all other impressions have to be translated before their meaning can be known. It is certainly true that a highly elaborate tactual apparatus comes to be the uniform accompaniment of superior intelligence." As has often been said, the skin is the boun-

[1] Psychological Foundation of Natural Realism, AMER. JOUR. PSYCH., Vol. IV. No. 3, p. 429.

dary between the ego and the non-ego ; that all that is within is subject, and without is object. So in touch we come nearer to the "thing in itself," if you please, than through the medium of any other of the senses. We can not smell a *thing*, but only the gaseous particles given off by the *thing;* we can not come into contact with a *thing* through vision, but only with the vibrations of ether which are radiated *from* the thing; in taste pure and simple, we are made cognizant only of a thing after its dissolution, and not of the thing as a whole. So there comes to be less need of interpretation in the case of skin perception than in any other, and so less opportunity of illusion. Speaking of this point Sully[1] says: "Tactual perception in so far as it is the recognition of an object of a certain size, hardness and distance from our bodies, involves the least degree of interpretation and so offers little room for error; it is only when tactual perception amounts to recognition of an individual object, clothed with secondary as well as primary qualities, that an opening for palpable error occurs."

Herein lies a pedagogical principle far too infrequently used. In the years of childhood we feel the world of sensitive appearances nearer to us ; we live immediately with and in it; there is an intimate bond of living dependence which unites us to it, and which is only broken when we are removed from contact with things. The brook to the boy with his pantaloons rolled up and the water gurgling around his legs is far more a brook, far more real, than to the poet who only hears its gurgle, or sees it sparkle. This close and intimate relationship between subject and object which we get through touch has led men to say that if the facts of physiology were rightly interpreted, they would lead irresistibly to the conclusion that sensation is not confined to the brain, but is spread over the whole sensory system. Wherever there is a sensory nerve there may be a sensation.

Johannes Müller said: "We do not assert that the mental principle has its seat in the brain alone. It is possible for the mind to act and receive impressions by means of an organ of a determinate structure, and yet be present generally throughout the body. The mental principle or cause of the mental phenomena, the conception of ideas, thoughts, etc., can not be confined to the brain. It exists, though in a latent state, in every part of the organism." Or as Isaac Taylor said: "The mind is not, as we suppose, the prisoner of the attic story, but is the occupant at large of the entire animal

[1] Illusions, p. 49.

organization, acting in each part of the structure according to the purpose of each."

If we consider carefully the probable value of double cutaneous sensation to a child in giving to it the basis for a separation of the ego and non-ego, we shall see without doubt that this simple and oft-repeated experience is of far-reaching significance. When a child first takes hold of something external to itself, if there be but a single sensation or set of sensations fused, there would not of necessity grow up a notion of otherness. But it seems to me, this basis for otherness has its origin in the double sensations it receives when it touches and handles its own body. If it takes its foot in its hand there are two wholly different sets of sensations presented simultaneously, and because of their unlikeness there must come a feeling, or consciousness of otherness, which in no wise could come through any set of single impressions. Later in life we come to use the special senses more and more and the skin sensations are left to form the greatest part of the semi-conscious state. When we stop thinking, and just *feel*, we find ourselves in a flood of sensations coming from the skin. While it is in a measure true that we are accustomed to neglect all of these sensations, notwithstanding they are pouring themselves in upon us and in no small measure constitute the mood, affect the temperament and put the attention in a state of uneasiness which demands and requires more stimulus to prevent mind wandering than when the subconscious is minimized. "We are most fully conscious," says Bastian, "when we are most receptive of external impressions, and we lapse into a completely or partially unconscious condition, when the advent of such impressions is for a time prevented, or when we are intensely absorbed in some train of thought, that is, when the activity of other portions of the cerebral hemispheres in some way dwarfs or eclipses that of the sensorial regions proper."

It is, to say the least, an interesting notion of Lotze [1] that whenever we bring a foreign body in contact with the skin, the consciousness of our personal existence is prolonged into the extremities and surfaces of this foreign body. According to him a tall head-dress is worn preferably because it lengthens our own "personality," producing the pleasing illusion that we ourselves extend up to that point. Thus we can understand the "disposition in low stages of culture, perfected afterwards in higher ones," to wear plumed helmets, waving and lofty coiffures. These fortified the consciousness of the wearer with the feeling of a majestic upward ex-

[1] Microcosmus. Eng. Trans. Vol. I. p. 592 et seq.

tension of his personality, as well as served to increase the fear-inspiring or respect-inspiring effect of the figure on others. So, too, high heels and stilts, while they raise us off of the ground, do not apparently separate us from the ground. The cane to the blind man becomes merely a prolongation of his arm and the touch at its end renders him directly conscious of the distance and quality of the object with which it comes into contact. The second class of these feelings is derived from all hanging and waving drapery and ornament, which "causes us to feel as if we were ourselves present" in the floating, flapping folds and streamers. This notion, he thinks, is the key to the uses of sash and ribbons, girdles, bracelets, trains, and all manner of wearing apparel of this sort. Thus the light gauzy stuff with which the maiden drapes herself is intended not only for the graceful curves that touch the skin in a few points, so as to charm others, but she herself feels that she is present in all of these movements which she "distinguishes as to the breadth, lightness and softness of their sweep." To Lotze all this was not mere theory, but a fundamental law of dress. For he says he has performed for the exact science of dress the same service as Kepler did for astronomy.

Then again, perhaps much of our æsthetic life has a closer connection with the skin sense directly than we are wont to think. There is without doubt truth in Lotze's[1] idea, when he says: "Accordingly it [the vesture] serves to make obvious to the senses the peculiar mobility of the figure and likewise gives to it, by means of the arrangements of its hanging folds, a semblance of the feeling which the figure itself preserves through its surroundings and its carriage, and in which it delights." And again he says in the same volume: "The pose of the figure in plastic art will always have for its principal purpose to show not only the complete adaptability with which the body obeys the spiritual impulse, but also that with which every single part by corresponding counterpoise, tension or relaxation compensates for the displacement of every other, so that the body appears as an extremely sensitive and elastic system of parts which keeps itself in equilibrium."

It is a noteworthy fact that illusions and hallucinations of the insane come through the sense of touch far less frequently than through sight or hearing. According to Griesinger,[2] in 177 cases of hallucination, occurring mostly in sane people, the following relations occur in the senses: Sight alone, 78;

[1] Outlines of Æsthetics. Trans. by Prof. Ladd, p. 76.
[2] Mental Diseases, p. 81. et seq.

sight and hearing together, 46; hearing alone, 16; sight, hearing and touch together, 4; with touch alone, only 9. He adds that diminution or complete suppression of the sensibility of the skin to impressions of temperature or pain is by no means frequent, still less is it general in insanity. Snell found that in 180 patients less than a score were anæsthetic, and where anæsthesia was present it occurred in cases offering very little hope of recovery. "When we read the history of trials for witchcraft," says Dr. Michéa,[1] "we observe that the inquisitors attached a high value to the existence of *cutaneous anæsthesia* as a sign of demoniacal possession. When an individual was charged with the alleged crime, the experts, after having bandaged the eyes, passed a magnifying glass over all parts of his body, previously shaved, with a view of discovering the marks of Satan (stigmata diaboli). The slightest spot on the skin was probed with a needle. If the puncture did not cause a painful sensation, if it provoked no cry or movement, the poor creature was a sorcerer and condemned to be burnt alive. If, on the contrary, he felt the wound, he was acquitted."

The fundamental character of the skin perceptions appears in a curious and interesting way in the Law. It is a well established fact that "mere words will not make an arrest." That is to say, the officer must touch the person whom he wishes to arrest in order to make a binding arrest, unless in some way the person, upon whom the writ is served, submits and shows himself subject to the authority of the officer without being touched. This will be made clear from the following decisions: "Bare words will not make an arrest, but if the bailiff touch the person, it is an arrest and the retreat a rescuous. On a motion for an attachment against three persons for a rescuous of a person taken in execution, it was objected that there had not been a legal arrest, as the bailiff had never touched the defendant. The court said this is a good arrest; and if the bailiff who has a process against one says to him when he is on horseback, or in a coach, 'You are my prisoner, I have a writ against you,' upon which he submits, turns back or goes with him, yet it is an arrest because he submitted to the process; but if instead of going with the bailiff, he had gone or fled from him, it could be no arrest unless the bailiff had laid hold of him. . . . . It is not necessary to show the warrant, or to tell at whose suit you arrest him, unless he demand it. And if you have two warrants in your pockets against him and produce neither, if he be rescued, either party at whose suit the warrants were made out may bring

[1] P. Gray. "Chirurgia," 1609, lib. VII. c. 10 (quoted from Winslow).

an action against the rescuers." In another old case,[1] it was decided: "Genner, a bailiff, having a warrant against Sparks, went to him in his yard, and being at some distance told him he had a warrant, and said he arrested him. Sparks having a fork in his hand keeps off the bailiff from touching him and retreats into his house. And this was moved as a contempt. It was held: "The bailiff can not have an attachment, for here was no arrest nor rescuous; bare words will not make an arrest, but if the bailiff had touched him, that had been an arrest, and the retreat a rescuous, and the bailiff might have pursued and broke open the house, or might have had an attachment or a rescuous against him; but as the case is, the bailiff has no remedy but an action for the assault, for the holding up of the fork at him when he was within reach is good evidence."

Again:[2] "I have not been able to find any real conflict between English and American authorities as to what constitutes an arrest. By all the authorities, a person may or may not be arrested without a manual or actual touching by the officer. Bare words alone will not make an arrest if the party resists the arrest." Many other cases might be cited, but so far as can be determined the same principle is held to in all of them, viz.: "The question as to whether or not an actual physical caption or manual touching of the body of the prisoner is necessary to effect an arrest depends upon the further question, whether or not the arrest is submitted to. If the party resists or endeavors to evade it, there must be an actual touching of his person, or some physical restraint of his liberty to depart, in order to complete the arrest; mere words will not do in such cases."

The importance of this whole matter for psychology depends upon whether touching, as used in arrest, is a survival of the attempt to take the prisoner by force, or whether it be that touch is here recognized as that sense through which errors or misunderstandings are less liable to occur than through any of the other senses. To consider it as a survival of the employment of physical force, seems by far the more natural explanation, and at first thought there seems to be scarcely any other way of looking at it. But the question comes, how would an officer arrest a blind and deaf man, or even a deaf man who could not read, except he make known the arrest through touch? This sense is never lacking completely during normal conscious life and so is always an open avenue through which the outer, or objective, becomes inner or subjective.

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[1] See Genner *v.* Sparks; 1 Salk. 79.

[2] Searls *v.* Viets, 2 Thomp. & C (N. Y.), 224, Potter, J.

However, if it be true, as seems probable, that the touch now recognized in the law of arrest is a survival of a more forcible means of taking possession of the prisoner, its present signification is entirely different, and I am assured by high authority that it is used now to simply make known to the one arrested that he is now in the power of the officer, and there is no means of denying it, because he was made acquainted with the fact through placing his hand on him.

The *rôle* that touch plays in the expression of the emotions of love and friendship can scarcely be over-estimated. The psychological significance of this fact is seen in many words we use for friendship and love. The words, *attraction*, *affection*, and the phrase, *attached to*, illustrate this. As is readily seen, these words have for their fundamental meaning the notion of being bound in contact with the loved one, and so indicate that there is a desire in us for actual dermal contact in the expression of love and friendship.

Dr. Brinton, the eminent "Americanist," has called attention to the fact that the same fundamental significance of the words for friendship and love is shown in many of the Indian languages. With the Chipeways, for example, the root for the word love is *sak*, which means *to attach*, *to fasten*. The root for the same word with the Aztecs was *zo*, which means *to fasten* one *with* or *to* another.[1]

It needs no exhaustive research to see that with all peoples, their heartiest salutations and greetings, their deepest and most fervent feelings of love and friendship require tactual expression. It has been the custom at all times and among all peoples to shake hands, to embrace, to kiss when friends meet. True it is that these tactual expressions have taken a great many forms, but each of them makes use of some form of contact.

It would be a most interesting study to trace the racial and local variations of these tactual salutations with especial reference to their development and evolution. It seems to me that a careful study of these forms would not only indicate the gradual changes and development of such feelings as they express, but that these accompany the development of finer capabilities of dermal sensitiveness.

The Feugians and Australians hug each other when they meet. Among the ancient Jews this same custom prevailed, supplemented by a kiss. We read in Genesis XXXIII, 4, that "Esau ran to meet him (Jacob) and embraced him and fell on his neck and kissed him and they wept." One of the most universal customs among the East Indians, and the

[1] Essays of an Americanist. (Philadelphia, 1890), p. 410.

Pacific Islanders in general, is that of "nose rubbing," as the travelers are wont to call it. These peoples, when they salute, have the general custom of touching their noses together and perhaps snuffing or smelling of each other. The kiss seems not to prevail so extensively among peoples of a lower stage of civilization in their salutations as it does in the higher stages. The Orientals, as is well known, have an elaborate system of salutations, in which the kiss plays an important part. Indeed, this method of salutation appears to have been practiced from the earliest antiquity by Semitic and Aryan peoples. If we should ask ourselves why we "shake hands," the answer, it seems to me, is not found in the usual explanation that we then see the one whom we meet does not carry a dagger, but in the demand for tactual expression of friendship. It is almost amusing to see to what inconveniences we will submit ourselves merely to grasp the hand of a friend and in return experience pressure from his hand.

Almost innumerable citations might be made showing how the forms have varied, but those given are sufficient to illustrate the point in question. Although, as has been said, the chief purpose of these forms of tactual salutation is that of the expression of the emotions, yet the fact must not be overlooked that a mere grasp of the hand often furnishes us with data concerning the character of a person not obtainable in any other way. The complex sensations of warmth, of firmness of grasp, of passivity, etc., etc., which we derive from a handshake, in no small way affect our opinion of a newly made friend. With some, these dermal sensations are all powerful. A friend related to me an instance which came under his direct observation, in which a young lady refused to continue her acquaintance with a young man simply because, as she said, the touch of his hand gave her "the horrors." It seems that with each of us there is a certain delicacy of touch, a certain degree of warmth, a certain amount of pressure, best suited for personal satisfaction, and corresponding to our notion of the best and fittest.[1]

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[1] Since the above was written, I have found that Dr. Brinton has expressed the same idea in his recent book, "*The Pursuit of Happiness*" (D. G. Brinton, Philadelphia, 1893, p. 131). He says: "When we consider how slightly most sensations of touch excite subjective states of mind, it is remarkable that in response to one stimulant, they are among the most powerful known in nature. This stimulus is that of another personality. The most positive feelings of both aversion and attraction are those excited by physical contact of the naked flesh. This is why it has been accepted in so many countries as a sign and proof of amity. The savage Africans touch noses and the civilized European shakes hands or kisses the hand or the cheek.

II. Education of the Skin with the Æsthesiometer.

The important discovery of E. H. Weber concerning the great local differences in the discriminative ability of the skin for the compass points, marked an epoch in touch psychology. His work not only stimulated to careful anatomical studies in order to know more of the skin as a sense organ from the anatomical side, but it has led to studies whose object has been to determine the possibilities of education in the discriminative ability of the skin.

A. W. Volkmann[1] found that the distance between the compass points, placed at the minimum distance they could be felt as two on different parts of the body, could be greatly decreased for any given part by exercise. And not only did this education show itself with regard to the special territory exercised, but strangely enough the symmetrical part on the other half of the body was likewise very markedly increased in fineness for this spacial discernment. He found that this increase, however, was soon lost for both sides, when the exercise was given up, and that after a month's rest, the increase was wholly lost. Suslowa observed that if the part of the skin lying between the compass points be stimulated by means of an induction current, or some light mechanical stimulus, such as touching it with a pencil, the sensitiveness for localizing was increased. Funke[2] found, just as Weber did, that on the median lines of the body, the skin is not so discriminative for the compass points. He also found that, after a month's exercise of a portion of the skin on the median line between the shoulder blades, there was but little or no increase in the power to discriminate between the compass points.

This ability to localize, however, varies greatly with different people. Valentine found that corresponding points on different people varied, even to a difference of four-fold, as regards their ability to distinguish the compass points as separate. But he also found that the ratios between the numbers expressing the fineness of discrimination for different parts of the skin of one person was about the same as that of another, even though much absolute difference existed in their abilities to discriminate finely.

Speaking in a general way, the more mobile the part of the body, the finer the discrimination it possesses. According to Vierordt, the relative fineness of the sense of locality of a given point of the skin of a part of the body, is a function of

[1] Berichte d. Sächsischen Gesellschaft d. Wissenschaften, p. 38. f.
[2] Hermann's Handb. d. Physiol. III. ii. p. 38.

its mobility depending on the relative extent of the excursion which it makes in the movements of the parts concerned, around its own axis, and increases proportionally with its separation from the axis upon which the part turns. While the facts do not support this so-called law completely, it must be regarded as expressing in a general way approximate truth.

In order to test the work of Volkmann and others, I began and carried out a series of experiments with special purpose of determining (1) how rapid the increase in sensitiveness through practice would be under given conditions; (2) to what extent the transference of this increased power would be made; (3) whether the sensitiveness of the surrounding parts would be raised or not; and (4) the curve for the loss of the power thus gained, after the exercise of the part had ceased.

Method of Work.

The tests were made on two subjects, F. B. D. (a man) and C. W. D. (a woman). For F. B. D. an area of the skin 7 cm. square on the palm side of the lower part of the left arm, beginning about 5 cm. below the elbow, was selected; while for C. W. D. a portion of the skin of the right arm 5 cm. square, also on the palm side, half way from the wrist to the elbow, was taken. In making the tests an æsthesiometer was used, whose points were of ivory and moderately sharp. It will be seen, by referring to the table, that two sittings for each subject were taken daily (with the exception of a few days), practically at the same time each day, and extending over a month. In the beginning of the work both arms of each subject were tested carefully on equal symmetrical parts and the discriminative ability noted for each. After this ability had been determined for both arms of each subject, experimentation was confined to the right arm for C. W. D. and to the left for F. B. D. At each sitting from twenty-five to forty observations were made on each subject. The subject being blind-folded, the skin was touched with the æsthesiometer, and the subject asked to judge whether one or two points of the instrument were touching the skin. The operator was careful to present these two conditions indiscriminately and equally often. The subject was so situated that the arm was always at rest and not otherwise stimulated. The operator was careful when two points were presented to have them touch the skin as nearly as possible at exactly the same time, otherwise the discriminative power would be reduced to that of the time-sense for

impressions, or at least so confused with it that the results would be too complex and varied. Then, too, care was needed to so shift about over the part to be stimulated that the after-images would be as little as possible confusing. To this end it was necessary to work often quite slowly and not to make too many observations at one sitting. Besides, the effect of fatigue was plainly noticeable after a long series of observations and so it was important to notice this factor. The method of finding the exact symmetrical part on the arms consisted in inking afresh the boundary lines of the spot exercised and then, after placing the palms of the hands together, symmetrically pressing the arms together. Thus' the same sized area and exactly symmetrically placed was obtained. The same plan was used at first so as to obtain the relative ability of the two arms before either had undergone any special exercise.

RESULTS : The following table is a statement of the daily ability to discriminate, with the date and time of day given. The results are given in mm., and in all cases represent the distance between the two points of the æsthesiometer, where at least seventy-five per cent. of the judgments were correct. It will only be necessary to glance down the columns to see the great increase in the discriminative ability. As will be seen for C. W. D. the distance of the points to begin with was 21 mm., but the average distance of the last week was only 4 mm. For F. B. D. at the beginning of the work, the points must be separated 33 mm., while the average distance for the last week was only 3 mm.

TABLE I.

DATE.	C. W. D.		F. B. D.	
	A. M.	P. M.	A. M.	P. M.
October 11,	22 mm.	24 mm.	29 mm.	26 mm.
" 12,	19 "	20 "	33 "	15 "
" 13,	17 "	22 "	18 "	18 "
" 16,	17 "	17 "	18 "	16 "
" 17,	18 "	16 "	18 "	15 "
" 18,	15 "	18 "	13 "	10 "
Av. Sensit. 1st week,	18 "	19.5 "	21.5 "	16.6 "
October 20,	22 mm.	18 mm.	10 mm.	15 mm.
" 22,	15 "	12 "	16 "	13 "
" 23,	11 "	15 "	12 "	10 "
" 24,	12 "	12 "	10 "	13 "
" 25,	8 "	7 "	7 "	7 "
" 26,	10 "	11 "	5 "	5 "
Av. Sensit. 2d week,	13 "	12.5 "	10 "	10.5 "

October 27,	10 mm.	8 mm.	9 mm.	8 mm.
" 29,	5 "	8 "	7 "	7 "
" 30,	4 "	6 "	4 "	9 "
" 31,	4 "	8 "	4 "	4 "
November 1,	5 "	2 "	4 "	5 "
" 2,	5 "	4 "	5 "	4 "
Av. Sensit. 3d week,	5.5 "	6 "	5.5 "	6.1 "
November 3,	5 mm.	6 mm.	3 mm.	3 mm.
" 4,	5 "	5 "	3 "	3 "
" 6,	4 "	4 "	2 "	3 "
" 7,	4 "	3 "	3 "	1 "
" 8,	2 "	4 "	2 "	2 "
" 9,	5 "	3 "	4 "	2 "
Av. Sensit. 4th week,	4.1 "	4.1 "	2.8 "	2.3 "
Gen. Av. Sensitiveness,	10.1 mm.	10.5 mm.	10 mm.	9 mm.

It was thought at first that there would be developed quite a difference in sensitiveness in both subjects between the morning and the evening sittings, and it can be seen by reference to the table that there is considerable difference in the averages for the first week, C. W. D. showing a greater sensitiveness during the morning sittings, while the opposite was true for F. B. D. It is interesting to note that this variation quickly decreased with practice and that after the first week there is practically no difference, thus following what might be termed a general law in education, viz.: one of the first effects of practice is to eliminate accidental hindrances, to render the work more and more mechanical and so less subject to variations due to unconscious influences. By referring to the general average, it is seen that the completed results for the month retain this preference in a slight degree, but it must be understood that these differences are due to those that arose in the first week.

Perhaps it would be well to call attention here to the absolute difference in sensitiveness, as shown by the table, for the two subjects. A great deal has been said about the comparative sensitiveness of the sexes with regard to touch, though a very few of such statements have been based on the results of accurate experiments. It would be wholly unscientific for me to make any general statement in regard to this question, simply because my experiments have been limited to two subjects. However, for these two the following may be said: In the first place it must be remembered that the part of the arm chosen for C. W. D. (a woman) was absolutely more sensitive than the part of the arm chosen for F. B. D. (a man), because that of the former was nearer the wrist than that of the latter. Hence the difference in sensitiveness at the start betwixt the two subjects should not be construed to mean that one or the

other had the greater power of discrimination, but that the difference arose chiefly because of the different positions of the parts exercised in the two subjects. By keeping this difference in mind, it is clear that F. B. D. showed in a slight degree the greater sensitiveness of the two. That is, he began with a part of the skin of less normal discriminative ability and succeeded in reaching a slightly higher discriminative ability than C. W. D.

Bilateral Transference of the Effects of Education of the Skin. One of the chief purposes of this work, as has been said, was to test the transference of the effects of exercise of the skin on one arm to that of the corresponding part of the other. These tests were made, as exhibited in the table after a month's work, and after it seemed that the approximate limits had been reached with regard to discriminative ability. The method used in making the tests was as follows:

The æsthesiometer was set for first test at 5 mm. whenever both points were pressed against the skin, in order to test what was considered the possible limit for discrimination before any exercise in testing might render the part more sensitive. Out of forty-five observations C. W. D. made but eleven errors, thus showing clearly that even when the points were so near, they could be recognized as two in seventy-five per cent. of the cases The next day another series of forty-five observations was taken with the points separated by 10 mm., when it was shown that eighty per cent. of them could be judged correctly. The first test made for F. B. D. was likewise for 5 mm., and out of fifty observations eighty per cent. of them were correctly judged. In order to show more clearly the transference of the increased ability to the corresponding part of the opposite arms, the following table is introduced, which shows the ability of both arms before and after the month's exercise.

BEFORE EDUCATING.

C. W. D.				F. B. D.			
Date.	Time.	Left Arm.	Right Arm.	Date.	Time.	Left Arm.	Right Arm.
Oct. 10.	8 A. M.	21 mm.	21 mm.	Oct. 10.	8 A. M.	33 mm.	33 mm.

AFTER EDUCATING.

Date.	Time.	Left Arm.	Right Arm.	Date.	Time.	Left Arm.	Right Arm.
Nov. 10.	8 P. M.	5 mm.	5 mm.	Nov. 11.	8 A. M.	2 mm.	5 mm.

From the result of these tests it is plain that either the education had crossed over, so to speak, or that the general discriminative ability had been wonderfully sharpened. That it was not largely due to the latter was proved in the follow-

ing way: Tests were made on the neighboring parts of the skin to those educated at the end of the month's work and the discriminative ability found to be much inferior to that of the part exercised and its corresponding part on the other arm. It is no doubt true that the general sensibility was slightly increased, but just how much, it is somewhat difficult to say. Unfortunately no general tests were made before the experiments began, to test whether or not the sensibility had been raised for distant parts of the body, such as one might naturally expect from education of the attention for this special kind of discrimination.

Other evidence, however, unexpectedly came out during the course of the experiments to prove that the education was limited to the locality in question. As was stated above tests were made on the territory surrounding the exercised part and its corresponding part on the other arm, which not only revealed the fact there mentioned, but also a marked difference in the *quale* of the local sensations; more striking in the arms exercised, but distinguishable in the others also. So much so that the subject could locate with a fair degree of accuracy the boundary of the exercised part merely through this acquired quality in the local sign. This difference in quality is best illustrated by the difference in the quality of the sensations, easily recognized, which arise from touching a finger-tip and immediately afterward some point on the back of the hand. There is not only an immediate recognition of a difference in location, but there is a different feeling present in the two cases. By touching the tips of two neighboring fingers, or for that matter one on each hand, the difference in the quality of the *feeling* is much less marked than in the former case. The change in the quality of the sensation, as Wundt [1] remarks, "is gradual as we pass from one point to another on the skin," and especially on the less discriminative parts of the skin is the change slow. Thus it was that this difference in the quality of the sensations was especially noticeable on the arms, for the change was so marked and so rapid near the boundary of the territory exercised and its corresponding part on the other arm that it attracted the attention of both subjects.

Given this change in the quality of the sensations, the question naturally comes, how did it arise? Surely not in a change in the fundamental structure of the end organ, because the time required for the development of this qualitative difference was too short for such changes. It must be that this rapid change was due to functional changes in the end

[1]Vorlesungen über Menschen und Thierseele. Vol. I. p. 214.

organ. The greatly increased exercise of the part would naturally tend to a larger development of these organs and hence there would come a quicker response to the stimulus, and especially a greater amount of nervous force would be liberated by a given stimulus. Wundt[1] says "the difference in the structure of the sense organ is the chief cause of the different quality of the sensation. Just as differences in tone and color fundamentally depend on the structure of the end organ in the ear and in the eye, so all qualitative differences which belong to any single organ have their necessary basis in the smaller variations, which may appear in the structure, or in the arrangement of the end organ."

The Rate of the Loss of this Increased Sensibility. Another purpose of this research, as stated above, was to determine as nearly as possible the rate of the loss of this increased sensibility, after the special exercise had ceased. It is clear that an exact curve of this kind can never be found, simply because the tests made to determine the exact state of the sensibility would naturally prevent the loss of the increased power as rapidly as it would occur without the tests. While, in a general way, it was known that the loss was rapid, no definite observations had previously been made, so far as I knew, on this special point. In determining how often the tests should be made in order to approximate the normal curve of such a relapse, there was, therefore, no help to be derived from well defined facts bearing on the question. It was decided, however, that the first test should be made one week after the special exercise had ceased, and let the results of this test aid in directing further experiments. It will be seen by the accompanying table that it was a mistake to wait so long before making the first test, for the decline at the first was very rapid, and decreased in rapidity inversely as the time. Later, tests were made more often, but varied in time, if, perchance thereby, a few points in the curve could be hit upon. But, as will be seen by the table given, the decline of this acquired sensibility was much more rapid than had been anticipated, and hence no definite curve can be given. The results here tabulated, however, give something of a glimpse of the true curve and in two places show that it was nearly reached. This table is of value chiefly as a guide to other workers in this line, though it is not without some value as an approximation to the facts sought.

[1] Op. Cit., p. 217.

TABLE SHOWING THE DECLINE OF THIS ACQUIRED SENSIBILITY.

For F. B. D.

Date.	Space Between Points.	Judgments. Right.	Wrong.
Nov. 10.	5 mm.	40	10
" 11.	10 "	42	8
" 19.	5 "	25	25
" 22.	10 "	35	15
" 26.	15 "	32	18
Dec. 4.	20 "	32	18

Explanation of Table. On November 11 the tests showed that when the points of the æsthesiometer were separated by a space of 5 mm., eighty per cent. of the judgments were correct; on the same day in the evening with the points at 10 mm. apart, eighty-four per cent. of the judgments were correct. On November 19, with no intervening exercise, it was wholly impossible to distinguish with the æsthesiometer set at 5 mm., and three days later less than seventy-five per cent. of the judgments were correct at 10 mm., and so on for the other days, as indicated.

Observations. Throughout the entire series of experiments, note was taken of all subjective experiences which it was thought might elucidate, or direct in the experiments. The following statements summarize the chief points noted:

1. Some time must elapse between two successive applications of the æsthesiometer, so as to give time for the after-images to die away, otherwise these will fuse with the primary image and render differentiation much more difficult.
2. The discriminative ability is less acute at the beginning and close of the sitting than toward the middle. After a few experiments at the beginning, the sensibility increases with an increased flow of blood to the parts, while toward the close there is a slight dulling caused by fatigue.
3. Under certain conditions, not yet fully determined, much greater pressure was required to render the sensations clear and definite.
4. The same amount of pressure on different parts of the arm gives very different degrees of pain. That is to say, the normal amount of pressure on a certain part giving no pain, would, if exerted on another place, produce a sharp pain. It seems that this difference is not wholly due to difference in the thickness of the skin, but to inherent difference in sensibility to pain.
5. The discriminative ability is finer when the two points of the æsthesiometer are pressed against the arm in a cross direction than when they are pressed lengthwise. (Kotten-

kamp and Ullrich found that this preference for the cross direction, for the upper extremities, over the lengthwise direction, was on the flexor side one-eighth, and on the extensor side one-fourth.)

6. Care was required during each sitting to keep the arm in a natural position, else the stretching of the skin rendered the sensibility abnormally acute.

III. Experiments on Open and Filled Space for Touch.

Tests with Active Touch. It is a well known fact that if two equal adjacent spaces are compared by the eye, the one filled and the other empty, the filled space will appear the greater. The purpose of the following series of experiments was to determine whether this would hold good for the sense of touch. That is, whether, of two equal spaces, the one offering the greater number of stimulations to the touch organs, as the finger is moved along through the spaces, would be judged the greater or not.

It is, perhaps, well to explain a little further here what is meant by open and filled space for touch. Of course, in a strict sense, there can be no empty or open space for touch. But by open space in this connection is meant a smooth, homogeneous surface, such as the surface of a bit of well-sized cardboard. By filled space we mean the same sort of a surface punctured from the under side, so as to offer to the sense of touch a series of additional and sharply defined sensations. The question arises here, how should these spaces be situated with regard to each other? Is there any source of error in the method adopted from Professor James, of dividing a line into two parts, marking a puncture at each extremity, and filling one part of this line with punctures and leaving the remainder for the open space, and thus be able to pass the finger over both spaces with one continuous movement? To illustrate, let the space between the dots at *a* and *b* in the accompanying diagram represent the open space, and that included between the dots at *b* and *x* represent the filled space:

$$\begin{array}{lllllllllllll} a & & & & & & b & & & & & & x \\ . & & & & & & . & . & . & . & . & . & . \end{array}$$

This question might be raised: If the tip of the finger be passed from *a* to *x* and the subject be asked to judge of the comparative lengths of the spaces *ab* and *bx*, is it not to be expected from a physiological reason that *ab* would seem shorter, because its length would be judged on the basis of the sensations received from the time the *last* sensitive point of the finger tip left the puncture at *a* until the *first* one

touched at *b*? We should answer yes, and were it not that exactly the same thing occurs in case of the filled space, there would be a permanent source of error in the method, even for relative judgments. That these relative judgments have not been influenced in this way is plainly evidenced by the fact that the apparent length of the filled spaces depends on the *number* of punctures. Then, too, it might be supposed that retardation of the movement due to friction as the finger-tip passed over the roughened filled space would have a tendency to make the filled space seem longer, but that this was not the case is the testimony of all the subjects, because the filled spaces were not so rough as to prevent free movement.

This, too, is proved by the fact that increase in the number of punctures, although increasing the apparent length of the space in most instances, did not increase the roughness of the space, but on the contrary made it more easy for the finger to pass over. With these apparent objections out of the way, let us pass directly to the problem.

Prof. James[1] says, "If one divide a line on paper into equal halves, puncture the extremities and make punctures all along one of the halves; then, with the finger-tip on the opposite side of the paper, follow the line of punctures, the empty half will seem much longer than the punctured half." He does not say how often this was tried, or whether the results obtained were the same, when the finger passed first over the punctured half and then over the open, as when it passed in the opposite order. Neither is there anything said of the absolute lengths of the spaces compared. Thinking that these conditions, if they were introduced into the experiment, might influence the results, I have carried on a series of experiments in the following way to determine the influence of these conditions:

Apparatus. (1) Eighteen cards were made according to Prof. James' method from well-sized stiff card-board and about the same width, with the spaces to be judged similarly placed for each card. (2) The spaces to be judged ranged from 2 cm. to 16 cm., as can be seen in the accompanying table. (3) A number of cards were varied only in the number of punctures in the same space. That is, the punctures in these cards were made closer together. The punctures were all made with the same point, and, on any given card, in the punctured space were equally separated from each other. It seems probable, at any rate, that whatever the results might be, the introduction of these variables would be

[1] Psychology, Vol. II. p. 241. (Loeb found the opposite to be true for rough and smooth threads. Pflüger's Archiv, Vol. XLI. p. 121.)

helpful in arriving at some explanation. (4) Almost an equal number of experiments were taken with each of the spaces presented first, for it was thought perhaps that the after-images of the punctures when presented first might appear to lengthen the punctured part.

The first series, that of C. W. D., was made on a wholly naïve subject, and at no time were the cards permitted to be seen or the purpose of the experiment mentioned. The cards were never presented in the same way two successive times, and the short ones were distributed so that no possible means of establishing an incidental and illegitimate means of judging was brought in. The other series given in the table were made on men who had thought and read more or less on the point, and could not be said to be ignorant of the purpose of the experiment. Their records, however, show that they judged according to their sensations, and hence they are in the main uniform.

TABLE II. Part 1.

COMPARISON OF OPEN AND FILLED SPACE THROUGH SENSE OF TOUCH.

a. Open Presented First. *b*. Filled Presented First.

			4		9		4		7		13		9		9		11		6	TOTALS.	
		2–2		2–2		$3\frac{1}{2}$–3		$3\frac{1}{2}$–3		$3\frac{1}{2}$–3		4–4		$4\frac{1}{2}$–4		$4\frac{1}{2}$–5		5–5		Open	Filled
C. W. D.	*a*	1	18	0	19	7	10	3	17	3	15	3	16	2	17	0	15	3	17	22	144
	b	1	18	0	17	4	14	4	15	2	16	1	13	3	13	0	12	4	12	19	130
J. A. B.	*a*	0	2	0	3	0	2	0	3	0	3	0	3	0	3	0	3	0	1	0	23
	b	0	2	0	2	2	0	1	1	0	2	0	0	0	1	1	1	1	0	5	9
J. H. L.	*a*	0	3	0	3	0	3	0	3	0	3	0	3	0	2	0	3	0	3	0	26
	b	0	3	0	3	2	0	1	2	0	3	1	1	0	3	2	1	0	2	6	18
J. S. L.	*a*	1	2	0	4	2	3	2	2	2	2	1	3	1	4	0	3	2	0	11	21
	b	0	2	0	2	1	1	0	2	0	2	0	2	0	2	0	2	0	1	1	16
J. A. H.	*a*	0	3	0	3	0	3	0	3	0	3	0	2	0	3	0	3	0	3	0	26
	b	0	2	0	2	1	1	0	2	1	1	0	2	1	1	0	2	1	1	4	14
H. A. A.	*a*	1	1	0	2	3	0	4	0	1	3	0	3	1	3	2	2	2	3	14	17
	b	0	2	0	2	0	2	0	3	0	1	1	0	1	2	0	2	2	1	4	15
F. D.	*a*	2	1	1	1	2	0	2	2	2	2	1	3	0	4	1	3	2	0	13	16
	b	0	4	0	2	1	3	1	4	0	4	0	0	1	4	3	1	4	0	10	22
Totals.		6	63	1	65	25	42	18	59	11	60	8	51	10	62	9	53	21	44		

Explanation of Table. Parts 1 and 2. The numbers 4, 9, 4, 7, etc., at the head and on the right of the double columns, indicate the number of punctures made in the number of cm. denoted by the number immediately below in the same column. Thus, in the first double column, the figure *4* at the head and on the right indicates that in the space of 2 cm. on the card there were four punctures; in the second double column the figure *9* indicates that in the number of cm. denoted

TABLE II. Part 2.

COMPARISON OF OPEN AND FILLED SPACE THROUGH SENSE OF TOUCH.

a. Open Presented First. *b.* Filled Presented First.

		11		13		7		10		19		12		13		14		16		TOTALS.	
		5–5		5–6		7–6		11–9		11–9		9–11		12–12		14–13		16–15		Open	Filled
C. W. D.	*a*	1	11	0	16	6	10	12	1	9	6	7	8	6	5	8	4	17	11	66	62
	b	4	8	2	12	3	11	8	2	7	7	6	7	9	2	7	6	21	2	67	57
J. A. B.	*a*	0	3	0	2	1	2	3	0	1	2	0	2	0	2	0	3	1	2	6	18
	b	1	0	0	2	2	0	2	0	1	0	1	0	0	0	0	1	1	0	8	3
J. H. L.	*a*	0	3	0	3	0	3	0	3	1	2	0	3	0	3	0	3	0	3	1	27
	b	1	1	1	3	3	0	3	0	3	0	3	1	2	0	3	0	3	0	22	5
J. S. L.	*a*	0	2	1	3	4	0	3	0	5	2	1	3	3	1	2	2	3	1	23	14
	b	0	2	0	2	1	1	2	0	2	0	0	2	1	1	1	1	1	1	8	10
J. A. H.	*a*	0	3	1	2	1	2	1	2	1	2	0	3	0	1	0	3	0	2	4	20
	b	1	0	0	2	2	0	2	0	2	0	2	0	2	0	2	0	2	0	15	4
H. A. A.	*a*	0	2	0	4	2	1	2	1	2	1	1	3	1	3	0	3	0	3	8	21
	b	0	1	0	1	2	0	2	0	2	0	2	0	2	0	2	0	2	0	14	2
F. D.	*a*	1	0	1	3	4	1	4	2	3	3	1	4	0	2	1	3	0	3	15	21
	b	1	0	3	1	4	0	4	0	4	1	4	0	2	0	3	1	2	2	31	5
Totals.		10	36	10	56	35	31	48	11	43	26	28	36	29	20	29	30	53	30		

by the figure immediatly below there were nine punctures, etc. Thus, in the first two double columns, the cards were exactly alike save in the number of punctures in the filled side, the one having but four, the other nine, punctures in 2 cm.

Results: With C. W. D., out of nineteen judgments between an open space of 2 cm. and the same amount of adjacent space punctured with four holes equally separated, eighteen judgments were made in favor of the punctured space being longer, to one in favor of the open space being longer when the open was presented first, and exactly the same proportion when the punctured was presented first. In the second column the same length of spaces was used (that is, 2 cm. for the open and the same for closed), but instead of only four punctures there were nine in the filled space. Here it will be seen that the filled space was judged to be longer every time for C. W. D., and by referring to the accompanying table, it will be seen that out of a total of sixty-six judgments for this card sixty-five of them were in favor of the filled space being longer. In the next three columns, that is, in the third, fourth and fifth, are tabulated the judgments rendered on three cards, all of which were exactly alike save in the number of punctures in the filled space. It will be seen, by referring to the totals in these columns, that increase in the number of punctures in the same space increased the number of judgments in favor of the filled space being the longer. Thus C. W. D., in the

first column of this set, made seventeen judgments; ten were in favor of the filled space being longer, and seven in favor of the open. In the second column of the sets twenty judgments were made, seventeen in favor of the filled space and three in favor of the open. In the third column of the set eighteen judgments were made, with fifteen for the filled and three for the open. By glancing at the totals at the bottom of these columns, it will be seen that these proportions were more than borne out by the other subjects. That is, the totals show that with the increase in the number of punctures in the same space, the space appeared longer even in the third column of the set. These judgments, it must be remembered, were between two spaces: $3\frac{1}{2}$ cm. of open space and 3 cm. of filled space; so that, even if the number of judgments were equal for the two, it would show a preference for the filled space. But when it is seen that under these conditions, the totals show the ratios of preference to be $1\frac{2}{3}$, $3\frac{1}{3}$ and $5\frac{1}{2}$ for the filled spaces respectively, it makes a clear demonstration, not only that filled space seems longer for active touch, but that the greater the number of separate sensations received in the filled space, the longer it appears to be. Now, it is clear that this increase in the apparent length of the filled space would not indefinitely increase with the increase in the number of punctures, but that there would be a point reached where these would not be separately distinguishable and the number of the sensations would diminish rather than increase, so that we should have a right to expect a decrease in the number of judgments preferring the filled rather than the open as the longer.

This same influence of the number of punctures, or in other words, the number of distinct sensations received in passing over the same amount of space, is seen by comparing the last double column of judgments in Part 1 with the first in Part 2. Here the spaces in each case were the same; that is, 5 cm., but in the former there were but six punctures in the filled space, while in the latter there were eleven. By glancing down these columns, it is plain to see that the filled space in each case was preferred, but much more often relatively in case of the card with eleven punctures. The totals show the ratios of the judgments to be two and one-tenth and three and two-fifths respectively. That is to say, there were two and one-tenth judgments, in the first case, in favor of the filled to one in favor of the open, but in the second there were three and two-fifths to one in favor of the filled. It seems that no further explanation of the results bearing on this point need be made, but that this is seen to be true, within certain limits, in all parts of the table. However, it is clear that as the

spaces to be compared increase in length, the influence of the filled space is less and less marked. That is to say, when the spaces to be compared are more than 10 cm. in length, the illusion does not hold so steadily. For example, the open space is judged longer more times than the filled where each space was 12 cm., although the filled had thirteen punctures. It is evident that, with the longer spaces, the subjects were guessing quite largely, though in a general way the filled spaces were preferred for the longer. The notes I made at the time of making the experiments are valuable here. With no exceptions, the subjects complained of the length of these longest cards, constantly saying they had no sure basis for their judgments, often saying that the judgment was a mere guess. Especially, they complained of the long open space as being so indefinite, because, in passing the finger over it, there was a sort of anxious effort to guide the finger through this blank, so to speak, in order that it would strike clearly the first puncture of the filled space. The hesitancy and caution here introduced would have a tendency to make these longer open spaces appear longer than they otherwise would, and in this manner in a slight degree, at least, vitiate the results. However, as has been said, even, in a general way, the longest open spaces used do not seem so long as equally long filled spaces.

The next question is, what difference in the judgments arose when the open space was presented first as compared with those where the filled came first? Or, in other words, when two equal spaces are to be compared as to their length by touch, one filled and the other open, does it make any difference in the judgments which is given first? This question can be answered by saying that when the spaces to be compared are short, less than 5 cm. each, there is practically no difference, but that above this, as far as we went, there is quite a difference, though varying slightly with different subjects. By examining the double columns of totals on the right of each of the foregoing tables, these statements are made more clear. Take, for example, the first subject again, C. W. D., it will be seen that in Part 1 the whole number of judgments rendered in favor of the open being the longer was twenty-two when the open was presented first, and nineteen when the filled was presented first, and 144 and 130 respectively in favor of the filled as being the longer. Of course, twenty-two and nineteen can not be directly compared unless the whole number of judgments in the two cases were the same. As this was not the case, the ratios of the judgments in the two cases should be compared and these are found to be the same, viz.: one to four and five-tenths in

each case. By following down the columns further in the same part, it will be seen that for some subjects there seems to be quite a difference, even when the spaces were less than 5 cm., but as there were comparatively few experiments made on each of these subjects, the difference ought not to count for too much. But in Part 2, it will be seen that for every subject save one, when the filled space was presented first, there was an increase in the number of judgments preferring the open spaces as the longer. Just why this should be so is not so easy to say. But it is perhaps due to the rapidity with which the sensations die out as the finger is moved through the spaces. Here again, the notes made during the progress of the experiments are helpful in making an explanation. It was observed that the longer the spaces compared, the greater the number of trials that were required for the subjects to come to a decision. Not only this, but each of them remarked that the long ones were very indefinite, and that by the time the finger was passed over both spaces, no definitely clear notion of the length of the first one was left.

Tests with Passive Touch. The problem of getting regular and steady motion of the proper rate for passive touch occupied much of my time before it was finally and satisfactorily solved. The conditions which it was necessary to satisfy were: (1) That the motion should be of uniform rate; (2) that it should be unaffected by any slight friction, and (3) that the card should thus be brought in contact with the finger with sufficient and regular frequency. Happening to be in the shop of the university mechanic one day, I noticed that in the movement of a planer for metals, which was run by a steam-engine, the above conditions could be easily fulfilled. Its movement was horizontal and could be regulated as to speed and frequency of return, while at the same time the apparatus was so massive that the motion would not in the least be influenced by a small amount of friction. All that was required, then, in order to make this serve the purpose, was to arrange a stationary arm rest, and to fasten a rack to the moving bed of the planer to hold the cards. Having made these arrangements, the conditions were then fully satisfied and the same tests could be made with the cards for passive touch that had been made for active touch.

The subjects were seated at right angles to the moving bed of the planer with the arm at rest. By simply extending the finger tip over the end of the arm rest, it was thus brought into contact with the punctures of the moving card. In this case, as in the tests with active touch, the finger-tip was kept in contact with the card only when the motion was in one

direction. To do this, the finger was raised from the card during the backward motion of the planer. The subject thus had opportunity to try as often as he wished before coming to a decision as to which of the two spaces was the longer.

In all the tests with passive touch, the cards were presented with the open space always coming first. It was deemed unnecessary to take any tests with the spaces reversed, because of the negative results with this variation in the tests with active touch. The subjects were blindfolded during the tests and directed to make their judgments wholly upon the basis of their sensations.

Under these conditions, as stated, it is very clear that all aid in determining the relative lengths of the two spaces, coming from the sensations derived from voluntary movement, was eliminated. That is to say, the various judgments of the subjects were based only on the sensations of touch. Cards with punctures, as previously described, were used, because with these there was much less likelihood of introducing an element of pain. As soon as the points became dulled by repeated touchings, new cards were substituted, which thus kept the spaces constant and their relations unchanged.

The following table shows the judgments of the subjects there named, under the above conditions:

	$2\frac{1}{4}$–2		2–$2\frac{1}{4}$		$2\frac{1}{2}$–2		2–$2\frac{1}{2}$		$2\frac{3}{4}$–2		2–$2\frac{3}{4}$		3–2	
Y.	0	8	0	8	4	4	0	8	6	2	0	8	5	3
H.	0	3	0	3	0	3	0	3	0	3	0	3	1	2
T.	0	3	0	3	0	3	0	3	0	3	0	3	0	3
J.	0	5	0	5	0	4	0	4	2	2	0	5	0	5
Others.	0	2	0	2	0	2	0	2	0	2	0	2	0	2
Totals.	0	21	0	21	4	16	0	20	8	12	0	21	6	15

	$3\frac{1}{4}$–3		3–$3\frac{1}{2}$		4–3		4–$2\frac{1}{2}$		$2\frac{1}{2}$–4		4–$3\frac{1}{2}$		$3\frac{1}{2}$–4	
Y.	0	9	0	9	3	6	4	5	0	9	2	7	0	9
H.	0	3	0	3	1	2	0	3	0	3	0	3	0	3
T.	0	3	0	3	0	3	0	3	0	3	0	3	0	3
J.	1	4	0	5	3	2	5	0	0	5	2	3	1	4
Others.	0	4	0	4	0	4	1	3	0	4	0	4	1	3
Totals.	1	23	0	24	7	17	10	14	0	24	4	20	2	22

Explanation of Table. This table is identical in plan with the one given for active touch, save in this one there is no specification of the number of punctures in each filled space. The number was not indicated, because the punctures were made equally far apart in each case.

Results. There were 312 judgments under these conditions, of which 270 were in favor of the filled space. This too, in spite of the fact that the open spaces of the fourteen cards used were really longer than the filled. If we consider the

judgments made, when the open spaces were always longer than the filled spaces with which they were compared, we will still find a great preference for the filled space. The number of judgments made from a comparison of the spaces on the eight cards whose open spaces are always longer than their filled spaces, is 178. Of this number the filled spaces were judged to be the longer 138 times, while the various open spaces were judged to be the longer only forty times. It is a waste of space and time to take up each card and show that in every case the totals give a greater number of judgments for the filled space. If the reader will only glance at the results of each column this fact will be manifest.

Tests when the Tactile Surface of the Finger-tip was Limited. It was mentioned previously that there might seem to be a fault in the method thus far used, in that when the entire width of the tip of the finger was brought into contact with the points, the notion of the length of the open space would be formed from the space passed over from the time the *last* sensitive point of the finger left the point at the beginning of the open space, until the *first* sensitive point touched the beginning of the closed space; thus, of course, making the open space appear shorter than it really was. While, as was said, it was thought that this was counterbalanced by the same thing happening with the closed space, to prevent any possible error in the results, the following variation in the method for passive touch was introduced. It is plain that if there be any fault in the method as indicated, it could only be remedied by limiting the amount of the tactile surface of the finger-tip to the space of a single sensory circle. This was accomplished in the following way: The arm-rest previously mentioned was lengthened so as to pass entirely across and above the moving card-rack; directly over this moving rack, a slot was cut in the board forming the arm-rest and into this slot a strip of tin was fitted so as to extend downward below the lower side of the board and approach the surface of the card. Through this tin, a hole was made just so large that when the finger was pressed against it, a portion of its sensitive surface, less than 2 mm. in diameter, protruded and came into contact with the punctures of the card passing along underneath. By this means, not only was the possible error in the results avoided, but all motion of the finger, either real or suggested, was wholly eliminated. All the conditions of this series of tests were the same as those in the previous tests, save the variations above described. It perhaps might be well to say here that punctured cards were used throughout the whole series of tests.

The tests made under these last conditions give the following condensed results: By using cards made and spaced exactly as those used in the tests with passive touch when the finger-tip was not limited, out of 200 judgments, only thirty-three were in favor of the open, while 167 were in favor of the closed being the longer, thus maintaining practically the same proportion of preference for the closed as in the tests with the other methods. These judgments were purposely obtained from subjects who had not served for me in the previous tests, in order to be sure that they be completely naïve.

The observation made by Herr Mellinghoff, as reported by Wundt,[1] to the effect that if the space between two points be divided into two equal parts by a dot, and then one of these parts be again divided into halves by another dot, the half of the whole space thus filled seems shorter for the eye than the open half, does not hold true for touch. To see whether or not this held for touch, I made a series of experiments with cards prepared to meet the above conditions, but found in the tests made thereon that the preference was as marked for the filled space under these conditions as it was in any of the previous tests for passive touch.

The question now arises, what is the meaning, from a psychological point of view, of these results? In the first place, it is plain that a closed space for the sense of touch, just as for that of sight, appears longer than an equal open space. Hence the explanations heretofore given, wholly based on the difference in time the eye used in passing over opened and filled space, must stop short of the real truth. That this illusion holds for touch is, I think, completely demonstrated by my experiments in passive touch, and in these the time used in passing over equal spaces was exactly the same. Hence, differences in this element cannot here enter into the explanation. The fact is, that in the comparison of these two separate trains of sense-perceptions, the one which was the fuller of definite and distinct sensations seemed the longer, though equal time had been consumed in the passage of each train across the focus of consciousness. All have noticed how much longer a road seems when first passed over, than it does after it becomes a familiar and often traveled road. The first time it is passed over, everything appears in consciousness, even to the minor details of the scenery, but as it becomes more familiar, there come to be only a few points, such as half-way places, or some peculiar objects, which are noticed at all, while the

[1] *Physiologische Psychologie*, 4th ed. Vol. II. p. 142.

stretches between are as open spaces. One cannot read Stanley's[1] description of the great African forest without feeling the vastness of it, chiefly because he makes one crowd into a small space of time, images of multitudes of plants and shrubs and trees and insects, and then reminds you that all you have in mind must represent only those things seen within a small radius, for you cannot even see through to the sunlight. Besides, he merely mentions and hastens, leaving the reader with only suggestions from which to bedeck the trees with their varied foliage and festoon them with myriad vines.

In the tests I made with the cards, it was not only found that of two equal spaces, the one open and the other filled, the filled one seemed the longer; but, also, it was found that of two filled spaces of equal extent, the one more completely filled seemed the longer. It must be remembered, however, that "more completely filled space" in this connection means capacity to present to the subject in a given time a greater number of distinct sensuous stimuli. It is clear that if too great a number of distinct stimuli be poured in upon the subject in a given time, they would drop below the threshold of differentiation, and such a train would be classed with those which arise from what we have termed open space. For example, if we run the fingers rapidly over a piece of coarse cloth or a wire screen, instead of a number of distinct sensations, there is but one continuous undifferentiated sensation; or if we glance over the lawn, the sensation is as if there were but one continuous stimulus, though we know it to be produced by the fusion of the stimuli from countless grass-blades.

The conclusion to which our results lead us, then, is that if we have given two trains of sense-perception produced in the mind in equal durations of time, the one which is made up of the greater number of distinct sensations requires of the mind a greater amount of space in its reference to the external world, provided, of course, that these trains are special at all.

It should be noted that this generalization says nothing of the origin of our notion of space, but only calls attention to the fact as exhibited in adult mental life. As to whether this be an inherent mental tendency or the result of habitual associations, the tests here made have nothing at all to say. There is thus offered abundant opportunity for speculation and hypotheses, but we prefer to run no risk in parting company with the truth and so will be content to await the outcome of a continuation of these investigations.

[1] In Darkest Africa, Vol. II. p. 72.

IV. Illusion for Weights. A Study in Association and Apperception.

It has long been known that of two bodies of equal weight, but of different size, the smaller one appeared the heavier when lifted. The purpose of the following series of experiments was to determine how the perception for certain weights could be influenced when (1) they were of the same *shape* and *weight*, but different in *size*, and (2) when they were of the same *size* and *weight*, but different in *shape*. The well-known experiments of E. H. Weber[1] and Fechner[2] will be recalled here. They experimented with weights of the same *shape* and *size*, but of different absolute *weight*. The purpose of these workers was to determine the fineness of discrimination for difference in weight. The work here described is of a very different nature from theirs, nevertheless the results obtained show the need of great care in the preparation of weights for such work as theirs and in so far is supplemental to their work. The purpose of this work, however, was to make a study in association and apperception by making use of one of the strongest associations we make in life, viz., that of two given weights of the same material the larger is the heavier. This association we have firmly fixed because of the often recurring and immediate sequence of the ideas. Indeed, if it could be said of any association, that it is inborn or racially fixed, it could be truly said of this one.

Tests on School Children.[3] Upon these a study was made concerning the illusions arising from comparing weights of the same shape and weight, but different in size. To make this study, I constructed some weights in the following way: A brass tube, a little over an inch in diameter, was cut into small tubes of the following lengths: No. 1, 1½ in.; 2, 2 in.; 3, 2½ in.; 4, 3 in.; 5, 3½ in.; 6, 4 in.; 7, 4½ in.; 8, 5 in. They were each made to weigh 132 grams by filling them with different substances and were all capped over with carefully fitted bits of brass. Care was taken to distribute the weight throughout the whole of each tube, so that one end

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[1]Der Tastsinn und das Gemeingefühl, Wagner's Handwörterbuch der Physiologie, 3, p. 543 et seq.

[2]Elemente der Psychophysik, I. p. 183 et seq.

[3]I am greatly indebted for the material here used to Mrs. Grace B. Sudborough, Principal of the Teachers' Training School, Omaha, Nebraska, who carried out under my direction this long series of tests on the public school children of Omaha. Her work of making the tests was of the most painstaking and careful kind and deserving of the highest commendation. Also, my thanks are due to Supt. Fitzpatrick, of the city schools of Omaha, for his interest and coöperation in the work.

should not be heavier than the other and thus introduce a disturbing element when they were lifted. There was nothing in the external appearance of any of them which would in any way suggest a difference in weight save their difference in size. It will be seen that the tubes increased in length regularly by half an inch and when set up they formed a series of regularly ascending steps. The design of this arrangement was to determine what effect such a relationship would have in suggestion so as to continue the illusion for the whole series. The suggestion thus made became so strong that, when the subjects began to see that the tubes could be arranged in a regular order, in many instances the judgments were changed and the tubes were made to occupy a place according to size. In some cases, however, this suggestion had the opposite effect; that is, it caused a disarrangement which would probably not otherwise have been made, simply, as the subjects afterward acknowledged, because they thought it improbable that the tubes had been made so as to decrease in weight so regularly. These, of course, were not strictly legitimate judgments, and should be cast out of the results wherever they are known to exist. But as there were very few of such they have not appreciably influenced the final results. In all cases where the subject permitted his judgments to be made from the sensations purely, the arrangement is probably without a variation, in order of their size. So with most of the subjects the illusion was strengthened by attracting their attention to this regular difference in size. Constant care was taken in making the experiments not to suggest to the subjects that they were given a puzzle to work, or that there was anything strange about the experiment. The tubes were put before them and they were simply asked to "arrange them in order of their weight." These words were used because it was thought they were freest from suggestion. The subjects thus began the experiment with the notion that it was a test in fineness of discrimination for weight. The tests were made under the most favorable circumstances possible. No one was permitted to be present but the operator while the tests were being made, in order to prevent, especially in the case of the younger children, any possible confusion or unnaturalness. They were directed to lift each weight in the same way, that is, by taking them between the thumb and finger and lifting them straight up, in order to prevent any physiological basis for a difference in sensations. No more restrictions were placed upon them, however, than were necessary to insure the introduction of no error. After the weights had been arranged in the order, as they thought, of their weight, they were

asked to make a comparison of the weights in relative terms, of the first and the last of the series. These comparisons have all been entered in the tables.

Before the work began, the children were divided into three classes according to their general intelligence. The brightest were grouped together to form class I, the good to form class II, and the dullest were put in class III. This classification was made by their teachers, and was unknown to the children. They were simply marked in the tables as belonging to a certain class. Also, their age, sex, nativity and physical development were noted, as well as the nativity and occupation of the parents. All this but the latter will be seen by a glance at any one of the tables given.

The following tables show the arrangements of the weights and their comparisons for the groupings with reference to grade of intelligence, sex and age.

CLASS I. BOYS.

Age.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Comparative weight of first and last of series.
7	1	2	3	4	5	6	7	8	2
8	1	2	3	4	5	6	7	8	3
8	5	4	2	1	3	6	8	7	3
8	1	3	2	4	5	6	7	8	3
8	1	2	3	4	5	6	7	8	4
8	1	3	2	4	5	6	7	8	2
8	1	2	3	4	5	6	7	8	2
8	1	2	3	4	5	6	7	8	3
8	1	2	3	4	5	6	7	8	2
9	1	2	3	4	5	6	7	8	2
9	1	2	3	4	5	6	7	8	3
10	1	2	3	4	5	6	7	8	5
10	1	2	3	4	5	6	7	8	6
10	1	2	3	4	5	6	7	8	4
10	1	3	2	4	6	5	7	8	2
11	1	2	3	4	5	6	7	8	3
11	1	2	3	4	5	6	7	8	5
11	1	2	3	4	5	6	7	8	$2\frac{1}{2}$
12	1	2	3	4	5	6	7	8	2
13	3	2	1	4	5	6	7	8	3
14	1	2	3	4	5	6	7	8	$1\frac{1}{2}$
14	1	2	3	4	5	6	7	8	2

Class I. Girls.

Age.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Comparative weight of first and last of series.
6	1	2	3	4	5	6	7	8	3
7	1	2	3	4	5	6	7	8	2
7	1	2	3	4	7	5	6	8	2
7	1	2	3	4	5	8	6	7	4
7	1	3	2	4	5	6	7	8	5
7	1	2	3	4	5	6	7	8	2
7	1	2	3	4	5	6	7	8	6
8	1	2	3	5	4	6	7	8	3
8	1	2	3	4	5	6	7	8	2
8	1	2	3	4	5	6	7	8	3
8	3	1	2	4	6	5	7	8	3
9	1	2	3	4	5	6	7	8	3
9	1	2	3	4	5	6	7	8	2
9	1	2	3	4	5	6	7	8	3
9	1	2	3	4	5	6	7	8	3
10	1	2	3	4	5	6	7	8	2
10	1	2	3	4	5	6	7	8	3
10	1	2	3	4	5	6	7	8	3
10	2	1	3	4	5	6	7	8	3
10	1	3	2	5	6	4	7	8	2
10	1	3	5	2	4	7	6	8	2
11	3	2	1	4	5	6	7	8	2
11	1	5	2	3	4	6	7	8	3
11	1	2	3	4	5	6	7	8	2
11	1	2	3	4	5	6	7	8	9
11	1	2	3	4	5	6	7	8	4
11	1	2	3	4	5	6	7	8	1
12	1	2	3	5	4	7	6	8	2
12	1	3	2	4	5	6	7	8	2
12	1	2	3	4	5	6	7	8	2

Class II. Boys.

Age.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Comparative weight of first and last of series.
7	1	2	3	4	5	6	7	8	3
7	1	2	4	3	8	7	6	5	2
7	1	2	3	4	5	7	6	8	2
7	1	2	3	4	5	6	8	7	5
7	1	2	3	4	5	6	7	8	3
7	2	1	3	4	5	6	7	8	4
8	1	2	3	4	5	6	7	8	3
8	1	2	3	4	5	6	7	8	2
9	1	3	2	4	5	6	7	8	5
9	1	2	3	4	5	6	7	8	6
9	1	2	3	4	5	6	7	8	6
9	1	3	4	2	5	6	7	8	3
9	1	2	3	4	5	6	7	8	2
9	1	2	3	5	4	6	7	8	3
9	1	2	3	4	5	6	7	8	4
9	1	2	3	4	5	6	7	8	2
9	1	2	3	4	5	6	7	8	2
10	1	3	2	4	5	6	7	8	2
10	1	2	3	4	5	6	7	8	3

Class II. Boys.—*Continued.*

Age.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Comparative weight of first and last of series.
10	2	1	3	5	7	6	8	4	1½
10	1	3	2	4	5	6	8	7	2
11	1	2	7	3	4	5	6	8	4
11	1	2	6	4	3	5	8	7	3
11	1	2	3	4	5	6	7	8	2½
11	1	2	5	3	4	6	7	8	3
11	1	2	3	4	5	6	7	8	3
12	4	1	3	2	5	6	7	8	1⅓
12	1	2	3	4	5	6	7	8	3
12	1	2	3	4	5	6	7	8	3
12	1	2	3	4	5	6	7	8	3
13	1	2	3	4	5	6	7	8	2
13	1	4	3	2	6	5	8	7	2
13	1	2	3	5	4	6	7	8	3
13	1	3	2	4	5	6	7	8	3
13	1	2	3	6	4	5	7	8	2
13	1	2	4	3	5	6	8	7	1½
14	1	2	3	4	5	6	7	8	3
14	1	2	3	4	5	6	7	8	8
14	1	2	3	4	5	6	7	8	4
14	1	2	3	4	5	7	6	8	2
14	1	4	3	2	6	5	8	7	3

Class II. Girls.

Age.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Comparative weight of first and last of series.
7	3	1	2	4	5	6	7	8	0
7	2	3	1	4	5	6	7	8	2
7	1	2	3	4	5	6	7	8	3
8	1	2	3	4	5	6	7	8	2
8	1	2	3	4	5	6	7	8	4
8	1	2	3	4	5	6	7	8	3
8	3	1	2	5	4	6	7	8	2
8	1	2	3	4	5	6	7	8	3
9	1	2	3	4	5	6	7	8	8
9	1	3	2	4	6	5	7	8	3
9	1	2	3	4	5	6	8	7	3
9	1	2	3	4	6	5	7	8	2
10	1	3	2	4	8	6	5	7	2
11	1	2	3	4	5	6	7	8	3
11	1	2	3	5	4	6	7	8	3
11	1	2	3	4	5	6	7	8	4
11	1	2	3	4	5	6	7	8	5
12	1	2	3	4	5	6	8	7	2
12	1	2	3	5	6	4	7	8	2
12	1	2	3	4	5	6	7	8	6
13	1	2	4	3	6	5	8	7	2
13	3	1	5	2	4	7	6	8	3
13	1	2	3	4	5	6	7	8	4
13	1	2	3	4	5	7	6	8	3
13	1	2	3	4	5	6	7	8	2
14	1	2	3	4	5	6	7	8	3
14	1	2	3	4	5	6	7	8	3
14	1	2	3	4	5	6	7	8	6
14	1	2	3	4	5	6	7	8	8
14	2	1	4	3	6	5	7	8	3

Class III. Boys.

Age.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Comparative weight of first and last of series.
7	1	2	3	4	5	6	8	7	2
7	1	2	5	3	4	6	7	8	2
7	1	2	3	4	5	6	7	8	3
8	1	2	4	3	5	6	7	8	3
8	1	2	3	4	5	6	7	8	4
9	1	2	3	4	5	6	7	8	4
10	1	2	4	3	5	6	7	8	2
10	2	1	5	3	4	6	7	8	2
10	1	3	2	5	4	6	7	8	7
11	1	2	3	4	5	6	7	8	2
11	2	1	3	5	4	6	8	7	2
11	1	2	3	4	5	6	7	8	2
12	2	1	3	4	6	5	7	8	3
12	1	2	3	4	5	6	7	8	2
12	1	2	3	4	5	6	7	8	3
12	1	2	3	4	5	6	7	8	2
12	4	2	5	3	1	6	7	8	2
13	1	2	3	4	5	6	7	8	2
13	2	1	3	4	5	7	6	8	4
13	1	2	3	4	5	6	7	8	5
13	1	2	3	4	5	6	7	8	3
13	1	2	3	4	5	6	7	8	5
13	1	3	2	4	5	6	7	8	1½
13	2	1	3	4	5	6	7	8	2
14	1	3	2	4	6	7	5	8	5
14	4	3	2	1	5	6	7	8	2
14	2	1	3	4	5	6	7	8	3
14	2	1	3	6	5	4	7	8	2

Class III. Girls.

Age.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Comparative weight of first and last of series.
6	1	2	3	4	5	6	7	8	3
7	1	2	3	4	5	6	8	7	3
7	1	3	2	4	5	6	7	8	2
8	1	2	3	4	5	6	7	8	2
9	1	2	3	4	5	7	6	8	4
10	1	2	3	4	5	6	7	8	3
10	1	2	3	4	5	6	7	8	2
10	1	3	2	4	8	5	6	7	2
10	1	2	3	4	5	7	6	8	3
11	1	2	3	4	5	6	7	8	3
12	1	2	3	4	5	6	7	8	3
12*	3	7	4	2	1	5	6	8	3
12	1	2	4	3	5	6	7	8	2
12	1	3	4	2	5	6	7	8	2
12	1	5	3	6	2	4	7	8	3
13	2	1	5	3	4	6	7	8	3
13	1	2	3	4	5	6	7	8	3
14	1	2	3	4	5	6	7	8	3
14	2	4	1	3	7	6	5	8	2
14	2	1	3	4	5	6	7	8	2
14	3	1	2	4	5	6	7	8	5
16	1	2	3	4	5	6	7	8	2½
18	1	2	5	4	3	7	6	8	3

*Very dull.

Explanation of the Tables. The Roman numerals at the head of the columns represent the arrangement of the order of eight weights. All the weights which were judged to be the heaviest were recorded in the columns marked I; those weights which were judged to be the next lightest, in the columns marked II, and so on to those judged to be the lightest, which was thus recorded in column VIII. The Arabic numerals entered beneath in these columns represent the weights which were judged to belong in this order of weight. Thus, the figures 1, 2, 3, 4, 5, 6, 7, 8, represent the weights in order of their size: 1 represents the weight 1½ inches in length; 2 represents the one 2 inches; 3, the one 2½ inches, and so on up to 8, which represents the longest one, 5 inches in length. So, when the figures correspond with the Roman numerals at the head of the columns, it means that their weight was judged to be in the inverse order of their size. The smallest was judged to be the heaviest, and the largest the lightest.

In the columns marked "comparative weight of the first and last of the series," the figures are entered which represent the number of times the weight entered in column I was judged to be heavier than that entered in column VIII. The figures representing any disarrangements of the weights in order of their size are printed in bold-faced type, in order that they may appear more distinct in the tables.

The Arrangement of the Weights. A single glance at the tables is sufficient to see that more than half of the children arranged the weights in the *exact* order of their size. By actual count, it will be found that ninety-two out of 173 arranged them so, and that of all the others, not *one* reversed them, or even made an approach to a reversal, notwithstanding it was supposed that with the younger children this would be the case. Then, the degree of the disarrangement is of much importance. What is meant by degree of disarrangement in this connection can be best explained by taking a concrete example. In the table marked Boys, Class I, the third boy has made the arrangement, 5, 4, 2, 1, 3, 6, 8, 7; here, in this case, weight 5 was judged to be the heaviest, and so on in the order they occur. Now, it is plain that his arrangement is much more a departure from the normal than it would have been had he placed them so that the weights would in no case be arranged more than one place removed from the column in which it would be normally placed. Instead of weight 5 being put in either column IV or VI, which would be a displacement of the *first* degree, it is put in column I, which makes it a displacement of the *fourth*

degree from the normal. The degree of displacement, then, can always be found by finding the difference between the number of the weight and the number at the head of the column in which it is placed. It is easy to see that, of all the displacements made, by far the greatest number are only of the first degree. In fact, out of 287, the whole number of displacements, 218 were of the first order, fifty-one were of the second order, thirteen of the third order, fourteen of the fourth, and one of the fifth. This fact shows conclusively that, without an exception, each child arranged them under the influence of the illusion that their weight, in a general way, was inversely as their size. This point will be touched again in the study of the results, with special reference to the age of the children.

Comparison of the Weight of the First and Last of the Series. In order to test the amount of the illusion produced, after the weights had all been arranged as the subject thought in the order of their weight, he was asked to judge in relative terms between the one he had placed as the heaviest and the one he had judged to be the lightest. By referring to the table, it can be seen that the illusion is quite large. The judgments of the comparative weights of the two vary from one and one-third to ten. That is to say, the weights in column I were judged to be as many times heavier than the ones in column VIII as the figures indicate in the column marked "comparative weight." The average estimate of 172 children is that the weights placed in column I, which was the smallest weight in all but twenty-seven cases, was three times heavier than those placed in column VIII, which was the largest in all but twenty cases. Before these tests were made on the children, some preliminary tests had been made on adults, and it was seen that these comparative judgments were likely to be larger when all of the weights were presented for arrangement, than if only two, the shortest and the longest, were given. That is, the same weights were judged to be nearer equal when they were presented without the intervening ones. To make a thorough test of this interesting difference, 178 children of the same ages, and as near as possible of the same intelligence as those to whom all were presented, were given only the two extremes, asked to arrange them in order of their weight, and, after this was done, asked to state, in relative terms, their notion of their weights. These last children not only did not see the intervening weights, but did not know that there were any. The tables for these are not given simply because in all but *two* cases the shortest is put the heavier, and the average of the

comparisons is 2.4. That is to say, the smallest one in direct comparison with the largest one, uninfluenced by the suggestion of the intervening ones, was judged to be only 2.4 times the heavier. It is quite clear, then, that we have in the case of the judgments, when all the weights were presented, a summation effect of the illusion which, therefore, did not depend directly on the difference in the size of the two weights compared, but on the suggestion caused by the previous judgments in arranging the whole series. That is to say, the subject, after having decided that weight one was heavier than two, and two heavier than three, and three heavier than four, and so on through to eight, would come to compare the weights one and eight with the notion built up that there was of necessity much difference between them. This illusion, thus built up unconsciously, is measured approximately by the difference between these two averages, 3 and 2.4, which is .6. This means that an illusion of seventy-nine grams was built up through this series, if the largest one be taken as the standard. For, since the weights all weighed 132 grams each, the difference between these averages would give an addition of seventy-nine grams to the smallest. If the smallest one can be taken as the standard, then the largest one would be estimated seventy-nine grams too light. It seems more natural to interpret this illusion in the latter way, because the subjects all arranged the weights, beginning with the heaviest, and thus necessarily making it the standard, frequently coming back to it, indeed, as a standard as they progressed in the arrangement of the series. It is a legitimate question for the reader to ask for an assurance of confidence in this difference between the averages, or better, to ask for the probable errors of these mean results. These probable errors have been calculated according to the rules given by Jevons.[1] The error thus computed for the comparisons, when all the weights were presented, is .06; that for the comparisons when only two weights were presented is .05. The meaning of this is that if any number of judgments be made under the same conditions, one-half of them will fall between 3.06 and 2.94 in the case of the judgments when all of the weights were presented, and between 2.45 and 2.35 in the case of the judgments when only the extremes were presented. We have thus arrived at an approximate measure of the degree of credibility of the average, which indicates the region for the truth to most likely hit upon. The truth in this case being that if any number of judgments be made under the same conditions,

[1] See The Principles of Science, W. S. Jevons. p. 387.

one-half of them would fall within the limits 2.94 and 3.06 when all the weights were presented, and within the limits 2.35 and 2.45 when only the extremes were presented. Since, therefore, there is a great gap between the approaching extremes of these two sets of judgments, we are warranted in asserting with a high degree of certainty that the significant difference between the two averages is not an accidental one, but represents a difference in the judgments based on some permanent difference in the psychological basis for the judgments. This difference, as we have said, is due to the suggestion which came from the intervening judgments when all the weights were presented.

It was thought at first that there might be some relation between the size of this illusion and the least perceptible difference between weights of this heft; for if the illusion which is measured by seventy-nine grams be divided by seven, the number of comparisons made in arranging the weights, the quotient would not be very far removed from the least discernible difference between weights of this heft, were the hefts of those compared of equal ratios. But the distribution of the errors shows that there is at least no discernible connection.

The question now arises, how can this illusion be distributed throughout the series? This can be answered partially by noting the position in the series where the disarrangements have been made. It is evident that wherever the apparent difference in weight between the weights was greatest, there would naturally occur less disarrangement from their order as to size, and *vice versa*. Then, it must be borne in mind, that, as the weights increase in size, the relative difference between adjacent weights, when placed in order of their size, decreases. Thus, the relative difference between the sizes of one and two is one-third, while between seven and eight it is only one-ninth. From this relation alone we would have a right to expect fewer disarrangements near the beginning of the series than we would near the end. But since there are more disarrangements near the beginning, as can be seen from a study of the tables, the relatively greater difference in size of the first ones was more than counter-balanced by the suggestion which came when it was seen that the arrangement was probably in the order of their size. It may be well to call attention here to what will be discussed more at length further on, and that is the fact that records on forty-eight adults show the reverse to be true. That is, as we might expect from the difference in the relative sizes as the series progressed, more disarrangements were made as this relative difference decreased.

Intelligence and Apperception. In the description of the method, it was stated that before any experiments were made on the children, that their teachers divided them, on paper, into three classes, with reference to their general intelligence: class I including the brightest ones; class II, those considered good, and class III, the dullest. This classification, of course, was wholly unknown to the children, as has been said, and so in no way would affect their tests. The purpose of this was two-fold: (1) To compare the strength of the associations of the three classes, and note the influence of these on the judgments, and (2) to determine if there be any difference in the suggestibility of the three classes.

1. The strength of the associations that have been made by the subjects between size and weight is found by a study of the arrangement of the weights. In the first place, no subject hesitated in deciding that there were differences in weight, and that of quite a marked degree. Indeed, the illusion is so strong that it is impossible to rid one's mind of it, even after one sees their weight is absolutely equal, providing that he perceives either through sight or touch that there is a difference in their size.

This last fact is a very striking illustration of the strength of a long continued and practically an unbroken series of associations. Throughout our whole experience, difference in weight is associated with difference in size, and especially so when the weights to be judged are apparently of the same material, as in this case. When the weights were placed before the subject, perhaps the first observation made was that they differed in size and that some were much larger than others. The next idea which arose, especially since the test of weight sensibility was involved, was to the effect that the largest was the heaviest. These ideas perhaps all arose in the mind of the subject before he had touched a weight, so that when he began to lift them, he would unconsciously put forth effort in lifting them in proportion to their size, as he had always been accustomed to do. But as the weights were of equal heft, they of course would respond the more readily to the lift, as they were the larger, and thus arose a basis for the illusion. One would naturally suppose that after the weights had been handled awhile, that the illusion would fade away, but, on the contrary, those subjects who took much time in the arrangement of them very rarely failed to arrange them in the order of their size, judging them to be lighter as they grew larger. Indeed, after having made them and worked with them for weeks, fully conscious of the illusion all the time, I could not rid myself of the idea that there was a striking difference in their weight, when I

depended alone on what seemed to be my sensations. Some of the adult subjects, upon whom I myself experimented, after being told, refused to believe that they were of the same weight, until they saw them weighed. And even then could scarcely prevent themselves from discrediting the balances.

I have nowhere found a better illustration of the power of apperception, or how our sensations are modified and transformed by the ideas already in the mind, than is brought out in these tests. The stimulus in the case of each weight was exactly the same, but the mental result for each weight was different. That is to say, were it not for the imposition of the associations previously formed, the weights would have all appeared equal in their heft, because the same amount of muscular effort would have sufficed in each case to lift the weights.

In Steinthal's[1] story of the party in the railway carriage, all of whom were strangers to each other, it is related that one member succeeded in telling the occupation of each of the others by their spontaneous answers to the same question. Here, as in the weights, the same stimulus was applied to each person, but unlike the results in the case of the weights, the responses were very different. This brings out very clearly the fact that from the same stimulus different results arise for different people, only in so far as the associations with the same special stimulus have differed. In the case of the weights, it has been, as has been said, a universal, almost an inborn association, that of two or more weights of the same material, the heft increases with the increase in size, and so not only was the stimulus the same for each person, but the sensations received were moulded and shaped under the influence of like associations, in such a way that we get the same general results. So, while the doctrine of apperception calls especial attention to the transforming influences, brought about by the elements which the mind itself furnishes, the association processes give the key to the influences thus brought to play upon the sensations.

2. If the foregoing analysis be true, then we would expect that the more intelligent the child, the more likely he would be to arrange the weights inversely as their order in size. For it would follow that those who have been the most discriminative, not only for differences in weight, but also for size and likeness of the material, of which the bodies to be compared were composed, have the strongest and, therefore, the most dominating connection between the two. Coming to a direct study of the tables for each of the three

[1] See James' quotation in his *Psychology*, Vol. II. p. 108.

classes, the truth of these statements is fully borne out. The following table will assist in making this clear:

		Number.	Degrees of Disarrangement from the Order of Size					No. of Disarrangements.	Av. No. of Disarrangements.
			1st.	2nd.	3rd.	4th.	5th.		
I.	Girls.	30	31	9	1			41	1.3
	Boys.	22	11	4	1	1		17	.7
II.	Girls.	30	43	8	1			52	1.7
	Boys.	41	56	10	4	2		72	1.7
III.	Girls.	23	31	14	3	1	1	50	2.1
	Boys.	28	45	6	3	1		55	1.9
Totals.		174	217	51	13	5	1	287	

Explanation of the Table. Here are gathered together all of the displacements from the order of size and exhibited for the three classes of intelligence. Each class is divided with reference to sex. In class I there were thirty girls and twenty-two boys; in class II, thirty girls and forty-one boys; in class III, twenty-three girls and twenty-eight boys. In the whole set of tests there were only five grades of displacement. It will be seen that nearly all of the displacements are of the first degree. That is, they were only one place removed from the place they would occupy in the series were they all arranged in order of their size. As the table indicates, there were 217 displacements of the first degree, fifty-one of the second, thirteen of the third, five of the fourth and one of the fifth. In the next column will be found the number of displacements, and in the next the average number of displacements for each child for each class. It will not only be noticed that of all the displacements for each class, there are relatively more displacements of the first degree in class I than in class II, and more in class II than in class III, but especially it will be seen that the average number of displacements per child increases as the grade of intelligence decreases, so that it happens that the absolute number of displacements for fifty-two of the brightest children is fifty-eight, while fifty-one of the dullest make 105 displacements, making the average in the latter case double that of the former.

In accordance with this result, one would have a right to expect a still smaller number of displacements when the tests were made on adults. And this is exactly what happens. The number of displacements for forty-eight adults is only forty-four, making the average but .9 of one displacement. (See the table of records on adults page 358.)

Difference with Regard to Sex. Here, again, we might expect to find that differences in the strength of the associations would assert themselves. Since boys have to do in

their experience with size and weight relations much more than do girls, we would expect their associations to be stronger between these two contiguous ideas. This is what we find to be true, though in no very striking degree. However, by observing the preceding table, it will be seen that the average number of displacements is less for the boys than for the girls in classes I and III, and equal to them in class II. By finding the average number of displacements for all of the boys and all of the girls when the whole series of weights was given, it is found that this average for the boys is 1.5, while it is 1.7 for the girls. Though this special difference is not at all marked, it is worth noticing. The chief difference between the judgments of the boys and girls, however, is shown in the difference between the comparisons of the first and last of the series when all the weights were presented and when only two were presented. The average for the girls when all were presented, is slightly larger than the same for the boys, as is also their probable error, showing that while the girls have a distinct notion that there is a difference in weight, they have not a distinct notion of what this supposable difference is. But when it comes to a comparison of the averages of their estimates when only two were presented, there is a much wider difference. When only the two weights were presented, the boys judged the small one to be twice as heavy as the large one, while the girls made an average estimate that it was 2.4 times as heavy. This brings out plainly—since the probable errors of these means are quite small—that the boys were more influenced by the suggestion than the girls when all the weights were presented, and so in line with the fact brought out previously in the paper, that those with the stronger associations of weight and size relations were the most suggestible, as shown by the size of their estimates of their comparative weights.

Difference with Regard to Age. The 173 children upon whom tests were made for the whole series of weights, were about equally distributed for the various ages from seven to fourteen inclusive. It was expected that there would appear quite a difference between the disarrangements of the weights for the two extremes of age, as well as in the comparative judgments of the two weights. But this is not what happened in either case; the difference in each case is too slight to point to any real difference between the ideas of the two for weight and size relationship. However, since only about twenty of each age were tested, no definite statement can be made with reference to what would appear if more were tested. This lack of difference between the records of

children seven and fourteen years of age indicates that the relationship of the ideas in question is about as definite, other things being equal, at seven as at fourteen. But this result, with respect to age, will only be of value as an indication of what can now be better determined. It seems unnecessary to give the special table here, upon which these statements are based, for they can be verified from the general tables previously given.

Just here it might be mentioned that in the whole number of children tested, no less than thirteen nationalities are represented. The fathers of forty-eight per cent. were born in the United States, twelve per cent. in Germany, eight per cent. in Sweden, eight per cent. in Denmark, five per cent. in England, four per cent. in Canada, three per cent. in Bohemia, two per cent. in Norway, and the remaining ten per cent. distributed about equally in Austria, Ireland, France, Russia, India and The Netherlands.

Tests on Adults. The tests recorded in the following table were taken before the work was done with the school children, and from this work the method was partially developed and completed. It may be of interest and value to say that the records here recorded were made on subjects of various occupations, including a number of specialists in psychology, pedagogy and mathematics, as well as druggists, mechanics, grocers, and those generally whose occupation would lead them to be discriminative with reference to weight and size. By glancing at the accompanying table it can readily be seen that there are fewer disarrangements for adults than for children. In the whole number of adults, which is forty-eight, there are but forty-four displacements; thus making an average of .9 of one displacement for each subject. But especially ought it to be noticed that all of these displacements are made by thirteen subjects, and that thirty-five of them, or more than seventy-two per cent., made no displacement at all from the order of their size. It should be borne in mind that throughout this whole research with the children as well as with the adults, *one* judgment, not in accordance with the order of the size, means in nearly all cases *two* displacements. Suppose weight four was judged to be heavier than three and so arranged; there would of necessity be two displacements, according to the method used. If it were possible to make sure of the number of judgments used in all the displacements, it would be better to compare on the basis of these. But this is not always possible and so the other method has been used. If there be any preference for either class in the method used, it has been to make the difference

less marked between them, by an exaggeration of the judgments of the most intelligent, not in accordance with the order of size. So, therefore, if such a method could be used, it would only accentuate more decidedly the correctness of the results obtained by our study of the records.

No.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Comparative weight of first and last of series.
1	1	2	3	5	6	4	8	7	
2	1	2	3	4	5	6	7	8	
3	1	2	3	4	5	7	8	6	
4	1	2	3	4	8	7	6	5	
5	1	2	3	4	5	6	7	8	
6	1	2	3	4	5	6	7	8	
7	1	2	5	4	3	8	7	6	
8	1	2	3	4	5	6	7	8	
9	1	2	3	4	5	6	7	8	
10	2	1	3	4	6	5	8	7	
11	1	2	3	4	5	6	7	8	
12	1	2	3	4	5	6	7	8	
13	1	2	3	4	5	6	7	8	
14	1	3	2	4	5	6	8	7	
15	1	2	4	3	5	6	7	8	
16	1	2	3	4	5	6	7	8	
17	1	2	3	4	5	6	7	8	
18	1	2	3	4	5	6	7	8	$2\frac{1}{2}$
19	1	2	3	4	5	6	7	8	2
20	1	2	4	3	5	7	6	8	2
21	1	2	3	4	5	6	7	8	$1\frac{3}{4}$
22	1	2	3	4	5	6	7	8	3
23	1	2	3	4	5	6	7	8	4
24	1	2	3	4	5	6	7	8	4
25	1	2	3	4	5	6	8	7	
26	1	2	3	4	5	6	7	8	3
27	1	2	3	4	5	6	7	8	3
28	1	2	3	4	5	6	7	8	3
29	1	2	3	4	5	6	8	7	$1\frac{1}{3}$
30	1	2	3	4	5	6	7	8	2
31	1	2	3	4	5	6	7	8	$1\frac{2}{3}$
32	1	2	3	4	5	6	7	8	4
33	1	2	3	4	5	6	7	8	3
34	1	2	3	4	5	6	7	8	3
35	1	2	3	4	5	6	7	8	3
36	1	2	3	4	5	6	7	8	3
37	1	3	2	4	5	6	7	8	3
38	1	2	3	4	5	6	7	8	$1\frac{1}{2}$
39	1	2	4	6	3	5	7	8	3
40	1	2	3	4	5	6	7	8	8
41	1	2	3	4	5	6	7	8	4
42	1	2	3	6	5	4	7	8	3
43	1	2	3	4	5	6	7	8	8
44	1	4	2	3	6	5	7	8	2
45	1	2	3	4	5	7	6	8	2
46	1	2	3	4	5	6	7	8	3
47	1	2	3	4	5	6	7	8	3
48	1	2	3	4	5	6	7	8	2

Results on Adults. It will not be necessary to explain at length what these tests indicate, for they are in line with the results obtained on the school children previously described. The method was the same in the two cases, except for the first seventeen adults who were not asked at first to make a statement in comparative terms of the weight of the first and last of their series. At this point in the work, the idea occurred, for it had become evident that the illusion was quite marked and that by this means I could get a measure of it. With the rest of the subjects, all of whom were males but two, the comparison is given. Let us first look at the arrangement of the weights. A glance is sufficient to show that there are much fewer displacements in this table than in those with the children. True it is that the twenty-two boys in class I have made a smaller average of displacements, but the probability is that if a greater number of these had been tested, their average would have been larger. Another difference between the displacements for the children and the adults has been mentioned previously; this is the difference of position in the series where the displacements occur. The following table will make this clear. Speaking generally, it is seen that the children make more displacements at the beginning of the series than toward the last, while just the opposite is true for the adults. This latter, as has been pointed out, is

TABLE SHOWING THE DISTRIBUTION OF DISPLACEMENTS.

		No.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.
I	Girls,	30	3	7	7	5	7	6	4	1
	Boys,	22	2	4	5	1	2	1	1	1
II	Girls,	30	5	7	8	6	9	7	6	4
	Boys,	41	3	10	10	12	10	8	11	8
III	Girls,	23	5	9	9	5	6	6	7	2
	Boys,	28	9	11	9	9	7	4	4	2
	Adults,	48	1	4	8	7	7	10	8	8

what one would expect from the gradually diminishing ratio of the sizes of adjacent weights. But this difference is not very striking and more records are necessary before any conclusions can be drawn from them. By far the most interesting difference that came out of the records of the children and those for adults is the greater difference which adults make

in the comparisons of the two weights when all were presented and when only the first and last were presented. The averages of the estimates when all the weights were presented is two and six-tenths, while that when only two were given is one and five-tenths. The probable errors of these means are very small, and thus render the result in a high degree credible.[1]

In brief, the results on adults show all the effects of more firmly fixed associations and hence a greater degree of suggestibility in the direction of these associations, as well as the greater influence of these in dominating and transforming the sensations directly received. That is to say, we are unable in this case to ascribe any definite value to the actual muscular sensations, simply because the preconceived notions in the mind, determined and fixed by all of our previous experience, render us unable to determine the truth in such conditions. This is a good illustration of what might be called unconscious dogmatism.

2. *On the Influence of Shape on the Judgment of a Series of Weights of Equal Weight and Size.* A series of tests was made on seventeen adults with weights of the same *weight* and *size*, but of different shape, to determine if this suggestion of difference in weight would prevail according to apparent difference in size. Eight weights were made of sheet lead, all of the same area and weight, but varied in shape from a circle to an irregular cornered figure. The table below shows that the weights which appear smaller, that is, are of a compact form, were judged to be heavier than those not so compact. While the preference, of course, is not nearly so uniform as it was for the brass weights, the averages of the numbers representing the order in which they were placed, show a decided preference. It will be seen, by reference to the table, that those weights which are judged to be the lightest are those whose form makes them appear the largest of the series. It should be noted here that when the weights were taken into the hands rather than grasped by the fingers, there was furnished a basis for a difference in the sensations of weight due to the fact that those more compact in form, for example the circle, exerted the same pressure on more closely related

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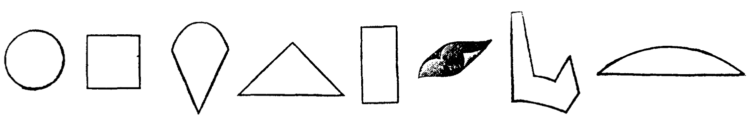
[1] Since my work was completed, my attention has been called to some experiments made by Charpentier, who used two brass balls of different size, but of the same weight. He found the same illusion, the larger seeming the lighter. His balls weighed 266 grams each, but the larger was judged to be sixty-six grams lighter than the smaller. Elements de la Sensation de Poids. *Archives de Physiologie.* Série 5, Tome 3, p. 126.

and fewer pressure spots, than those whose form was more irregular. The subjects were carefully observed on this point and very few of them permitted the introduction of this disturbing physiological element. Even when the weights were taken into the hands, the decision was almost invariably rendered after a balancing, of the two compared, by grasping them between the fingers. The avoidance of this physiological cause for illusion went so far with one subject (L) that he tied strings to the weights and then lifted them by the strings. Nevertheless, his record corresponds very closely to the general result.

No experiments were purposely made, either with the brass weights or the lead weights when the subjects were blindfolded, because it was thought that such a method would of necessity introduce much more strongly this probably disturbing physiological element, which was otherwise almost wholly eliminated, even when the subjects were not directed how to lift the weights, but left to use their own method, as was the case with the adults. It was, of course, wholly useless and unnecessary with either series of weights to take a series of experiments, when the elements of size and form were both eliminated, and the weight for each remained the same.



A,	1	5	3	2	7	8	6	4
B,	4	1	2	5	6	3	8	7
C,	1	3	2	4	5	8	7	6
D,	2	4	1	3	5	6	8	7
E,	3	4	2	1	6	5	8	7
F,	7	5	3	1	2	4	6	8
G,	3	6	1	4	2	7	5	8
H,	2	1	4	5	3	7	8	6
I,	2	1	5	3	4	6	8	7
J,	4	2	5	3	1	2	8	6
K,	1	2	3	4	6	5	7	8
L,	1	4	3	2	6	7	8	5
M,	1	2	3	5	4	6	7	8
N,	5	6	1	4	3	2	7	8
O,	3	2	1	6	4	8	5	7
P,	1	2	3	5	4	7	8	6
Q,	2	1	5	3	7	8	6	4
Totals,	43	47	47	60	75	99	100	112
Averages,	2.52	2.76	2.76	3.52	4.41	5.82	5.88	6.58

Explanation of Table. The figures drawn at the head of the columns roughly show the shape of the weights used. The comparative sizes must not be inferred from the drawings, as they are not exactly made. The figures in the

columns represent the order of their weight as the subjects thought. For example, the first row of figures is the order which A judged them as to weight; the second is B's order, etc. It will be noticed that the weights are arranged in the table in the order they were judged on the average. But it must not be inferred from this arrangment that they were presented to the subjects in this or any other order. They were placed before the subject in a hap-hazard way and the subject left to separate and arrange them according to his own method.

Conclusions.

1. The more intelligent the children, other things being equal, the stronger are the associations between the ideas of size and weight of a given material.

2. The stronger this associative element becomes, the more likely it is to dominate and pervert the true sensations, when the conditions are such that these associations do not hold.

3. The elements the mind furnishes, keeping these conditions, have far more influence in determining the judgment than the sensations directly received.

4. Illusions are easily built up when suggested along the lines of firmly fixed associations.

5. Consequently the brightest children are more suggestible under these conditions than the dullest ones.

6. The method used in this research furnishes a means of measuring suggested illusions of this type.

7. Adults have stronger associations between the ideas in question than children, and, despite the fact that they have a higher degree of sensibility for difference in weight, their sensations are more transformed and influenced by the element which the mind itself furnishes than are those of children.

8. Facts which vary, within limits, from our established habits of apperception are simply not taken account of at all; or, if on some occasion the conditions force us to see how our minds have become insulated against the reception of different relations, we do so with a wholly new feeling of personal fallibility.

9. The pedagogical significance of the facts emphasized in this research is of the utmost importance. It has to do with one of the most fundamental laws which regulate our mental life. The foregoing tests as a whole show how strong and dominating an association between ideas may become when they are practically unseparated and immediate in their sequence. "That which the law of gravitation is to astronomy," says John Stuart Mill, "that which the elemen-

tary properties of the tissues are to physiology, the law of the association of ideas is to psychology."

V. Minor Observations.

Reversal of Certain Cutaneous Sensations of Motion. The well-known "waterfall" illusion for vision, or as it is more scientifically called, the antirheoscopic illusion, suggested that perhaps the same illusion might occur in case of the skin, if practically the same conditions were complied with; that is, if the skin were stimulated rapidly and regularly in a continuous direction comparable to the stimulus given to the retina by the steadly falling water, or the moving stripes on the antirheoscope. To do this a frame was made sufficiently large to allow the arm to be placed in it. At each end of the frame a roller was placed and over these a belt of plaited velvet was fastened. One of the rollers was furnished with a crank, so that by turning this the skin could be stimulated with the moving folds of the velvet regularly and continuously in whatever direction desired. The folds in the velvet belt were about three-fourths of an inch in depth and one and one-fourth inches apart. Experience proved that these folds need to be made carefully and directly across the belt with no irregularities in them at all, otherwise the stimulation becomes too complex, and the resulting after sensation mixed and indistinct. Care needs to be taken, too, that as far as possible, the velvet must be kept of the same temperature as the arm, for if not, it will absorb the heat of the arm so readily as to swamp all other sensations in that of temperature. The folds of the velvet need to be pressed sufficiently hard against the skin so as to make the sensations of each fold clear cut and distinct. Having as nearly as possible satisfied these conditions, I tried the apparatus first on two women, both of whom were wholly ignorant of the purpose of the experiment, and knew nothing of the illusion for vision. I simply asked each one to give attention to the sensations caused by the moving velvet, and after it was stopped to notice carefully any sensations that might then come out different to those perceived while the belt was in motion. The result was that each one immediately said, "I feel the motion as if the belt were moving backward." Of course these trials were made so that the results of the experiment on one were unknown to the other. Afterward, I tried it upon myself and Dr. Sanford, and though each of us was somewhat skeptical as to the nature of the results, something like backward motion was experienced. It was tried for the lower arm lengthwise and crosswise, and in both directions for the

palm of the hand and for the forehead. For myself, the clearest results were obtained on the sole of the bared foot.

This sensation for reversed motion is not so clear and distinct for touch as it is in the case of vision, as would naturally be supposed, for our attention is so much less frequently directed toward sensations for touch than for sight, that we are unable to concentrate it so carefully and easily.

Although this experiment shows that the illusion in question can be produced in touch as well as in vision, no completely satisfactory explanation can be given. The following suggestions, however, may have something of truth in them and I give them merely for what they are worth.

Stimulation of any part of the body immediately causes a greater flow of blood to that part and especially do the smaller blood vessels become congested when the skin is stimulated with friction, as in the case of the foregoing experiment. Then as the folds of the velvet are swept over the skin there is a tendency for the blood to be forced along in the capillaries in the direction of the moving belt and when the actual motion stopped, to flow back again to restore the equilibrium in the vascular pressure. The rush of the blood to the face in blushing, which is very noticeable and the direction of the wave quite marked, will illustrate in a magnified degree what takes place in the skin under the conditions of the experiment. This view is in harmony with the fact that the sensation of reversal for touch as for sight is immediate, quickly dies out and does not return again as other after-images.

I am aware that the view here taken seems much more reasonable for touch than for sight, and yet the changes in circulation caused by the hastened metabolic processes in the eye brought about by such stimulation may suffice to restimulate the retinal elements of vision in a reverse order to that of the direct stimulus and thus furnish a basis for the illusion.

After-Images of Touch and Summation of Stimuli. Dr. Alfred Goldscheider[1] has called attention to the fact that if one touches the skin with a moderately sharp pointed instrument, the first sensation arising therefrom is a pricking sensation which soon dies away, but after this sensation has disappeared, a second sensation arises which retains something of the pricking sensation, but has lost the quality of touch which was prominent in the first. He found by further

[1] A. Goldscheider. *Ueber die Summation von Hautreizen.* Du Bois-Reymond's Archiv, 1891, p. 161.

experiment that if the primary sensation approached very near to a painful sensation, the secondary sensation became distinctly painful, while if the primary itself became painful, the secondary sensation was less distinct. It was found, however, that if the skin be stimulated with a single electric stimulus, the secondary sensation did not come out. It was found that, in order to arouse this secondary sensation, it required a number of electrical stimuli made on the same spot. A series of four stimulations gave a clear secondary sensation only on condition that certain intervals between the stimuli be retained. That is to say, the sensation was most marked with intervals between the stimulations of from .03 to .06 seconds. If the intervals were varied above or below these limits, the secondary sensation became less and less distinct until it gradually disappeared.

For the purpose of testing these results, I first made some small pencils of cork, gave one each to a number of subjects all in the same room, and all students of psychology, and asked them to touch their hands with these points and note carefully their sensations, allowing several seconds to elapse between the separate touches. I did not tell them what to expect, but simply to describe carefully their sensations; Goldscheider's work was known, however, to a part of them at least. The following are a few of the results obtained, and are practically the same as those on a number of other subjects: —

1(A). A more or less ticklish sensation, much like that of a mosquito bite that has nearly disappeared, at first came out. Some time after, when I supposed that all sensation had disappeared, a sharp, somewhat painful, sensation flashed out and disappeared nearly as suddenly. But after quite a period of time, in fact after writing the above description, it appeared again, but with less force each time, until it completely died away.

2(B). At first, a sense of contact and then a sense of pressure. These were both accompanied by a sensation of temperature. On touching my hand again somewhat more vigorously, I find that the demarkation between the sensations of touch and pain, is much more definite than I thought it to be. I noticed no after-sensations at all. [In later experiments, however, this subject had distinct after-sensations.]

3(C). After touching my hand with the cork, in addition to the feeling of contact, in about one-half of the cases, there came a distinct secondary sensation of a more or less painful character, and diffused over a greater area than the primary sensation. In the cases where the primary sensation was

somewhat painful, the after-sensations did not come out so clearly.

4(D). After touching the back of my hand the first time, as an after-effect there arose a slight drawing sensation ; the second trial, I struck my hand a little too hard so that the first sensation was slightly painful. Later a sharp, fine sensation, somewhat extended, but chiefly located about an inch nearer the wrist, and somewhat toward the little finger side, came as an after-effect. This was very distinct, both in quality and place, from the slight pain resulting immediately from the stroke. In the third trial the pressure was slight, and the sensation of a sticking character and seemingly not so fleeting as in the other cases; it faded slowly, but not regularly, and seemed to recur several times. In the fourth trial, the touch was again very light, and the after-image was clear and somewhat irradiated. I noticed that the after-images of former touches returned and rendered this last sensation somewhat vague.

From the above results, it is clear that after-images of touch are easily recognized, and that these are of a more or less painful character, even though the primary sensation itself contain no element of pain. My results, of which only samples are given, are in accord with those obtained by Goldscheider for touch. I did not repeat his experiments with electrical stimulation to obtain the summation effects, but I found that this after-image could be much sharpened and enforced by the effects of tickle. After experimenting some time on this after-image effect, it seemed that the clearness and vigor of its effect were due largely to the plethoric condition of the part stimulated. That is to say, when the hand, for example, was cold or had been resting passively, the after-image from a given touch was much less marked than when the hand was full of blood and the attention directed toward it. It occurred to me that the tickle sensations might serve to reinforce and bring out more distinctly this after-effect. Accordingly on many naïve subjects, I first made experiments to determine the normal strength of the after-image before tickle, and then in a short time stimulated the surface thoroughly by tickling. The result, in most cases, was a distinctly marked increase in the clearness and vigor of the after-image, especially in its painful quality. No difference was noticeable, however, in the quality or quantity of the primary sensation. It would seem, then, that increase in sensitiveness for direct touch could not be assigned as the whole cause. Furthermore, I came to see that the rush of blood to the spot pressed, after the cork point had been removed, coincided in time exactly with the recurrence of the after-sen-

sation, and in my own case, and even with other subjects, I could determine precisely when the after-sensation would flash out, by simply watching the rush of blood to the spot touched. Now, it is clear that this rush of blood is the chief, and so far as I can distinguish, the only direct stimulating agent for the production of this after-sensation. Just how this is done is an entirely different thing. It may be that the temporary lack of blood in the capillaries at the point touched, caused by the pressure of the cork point, was sufficient to permit an amount of metabolism of the part, great enough, so that when the blood returned and the opposite process was started, the toxic agents released acted directly on the delicate adjacent nerve tendrils, and the result was the after-sensation. This view seems the more probable when it is remembered that the after-sensation contains invariably a painful element and none of touch or contact. It may be, and probably is, true that also the minute tissues of the part are slightly ruptured, and the return of the blood to these would thus restimulate the adjacent nerves more sharply than the original stimulus, and hence the feeling of pain. Whether this view be correct or not, the fact still remains that the return of the blood to the point stimulated, is simultaneous with the after-sensation. Goldscheider's theory, that this summation stimulus is probably due to the hindrance and the reinforcing of the stimulus in the cellular tissues, which are spread out along the nervous tract and in connection with it, seems to me to go needlessly into difficulties.

Dermographism. "Dermographism, or skin writing, is said to be dependent on two causes: an irritable nervous system and some toxic agent. Both of these conditions become causes which may act either on the peripheral vaso-motor nerves, or on the vaso-motor centre, and so influence directly the circulation." The foregoing is about what the physicians say of this. But it is very easy to obtain upon perfectly healthy subjects. In order to test this, after having found it true for myself, I asked a class of ten men to write, with moderately blunt pieces of cork, on their lower arms the word *blood*, and then slightly rub the arm. This was done, and the word in each case came out in flaming red letters after a pause of a few seconds. In some cases, there was left a welt where the cork passed over the skin, after the increase of the blood had died away, although the writing with the cork was done very lightly and gave no sensations of scratching.

The studies here described were made during the present academic year at Clark University, under the immediate direction of President Hall, to whom I wish here publicly to express my grateful acknowledgments for the many suggestions received during the progress of the work. Likewise to Assistant Prof. Sanford, who has been ever ready to render any assistance, my thanks are due. To all those also who have so generously lent their services as subjects for experimentation, I acknowledge my indebtedness, and especially to Mrs. Dresslar, without whose coöperation some of the work would have been impossible.